

Psychological Bulletin

A fact has a peculiar and intricate structure. It belongs worlds, the world of objects and events, and the world of human course. Facts are invisible and inaudible. They can not be burned in furnaces or used to roof houses, or shot from big guns.

Objects and events are not facts; they are merely objects and events. They are not facts until they are described by persons. And it is in the nature of that description that the quintessence of fact lies. Only when an event has been given a very specific kind of description does it become a fact.

When we say, "Let's get down to the facts," what we are saying is much more than that we should look at or listen to or smell or touch real objects, or that we should all observe an event. What we are really proposing is that we all try to find certain statements on which we can all agree. Facts are the basis of human cooperation.

A fact is an event so described that any observer will agree to the description. There are, of course, no facts that meet this too general requirement. We are satisfied—we have established our fact—if any observer within the circle of persons with whom we discuss events will agree. There are always feeble-minded persons, ignorant persons, insane persons, apathetic persons, whom we disregard. There are, therefore, no absolute facts, and a universe without men and human discourse would be a universe without facts.

It may be readily granted that this definition of a fact is no fact itself. There are many men who would hold that facts are just events and objects, and will so continue to assert even after they hear this definition. The definition is made, however, with the hope that certain elite hearers will immediately accept it. They are the audience to whom the definition is addressed.

Psychological facts are events so described that any psychologist

will accept the description. This description is done perforce by psychologists and for the benefit of other psychologists. Laymen will not necessarily be familiar with the language itself, or with what should be looked at (for, after all, facts are based upon what is seen and heard and are not pure inventions of men.)

Facts are of particular importance to science. Science is founded on an interest in fact. By that I do not mean an interest in nature. I mean an interest in getting down to a factual basis which starts with descriptions acceptable to any observer. This agreement must not be limited to the members of a cult or the partisans of a cause, or the speakers of a language. Sun worshippers may all agree that the Sun God has reappeared with the new day. Members of other cults will not accept his divine attributes. But all men in any language will agree that the circular disc of light is again present.

The agreement that is essential to facts can not depend upon skill or judgment or taste, unless we have assurance that that skill or judgment or taste can readily be acquired with training. The expert taster, the connoisseur of pictures, the skilled diagnostician does not contribute to the advance of science unless he can discover a factual basis for his judgments open to all interested men.

As psychologists we face in the near future the onrush of a torrent of new facts. The number of psychologists has doubled in a few years and if we read the signs correctly is about to double again in an even shorter period. Even the present members of the Association will be contributing to the deluge if we can interpret the fact that during one year approximately three-fourths of us (three thousand of us in round numbers) have changed our addresses and presumably have encountered new persons and problems and new scenes. Industrial psychologists, school psychologists, personnel psychologists, clinical psychologists, will soon be filling the pages of our journals with statements which will, it is to be hoped, include the right proportion of facts. We hope to remain on a factual basis.

But that is not to be taken for granted. A flood of new publications is not automatically a flood of new facts. And it may include many facts which do not contribute materially to the science of psychology. Collections of facts are not science. They are the material out of which science can grow, but they are only the raw material of science, and sometimes they are not even that.

Psychological facts are events described in psychological terms and therefore by and for psychologists. The descriptions which facts require have not been lying about waiting to be noticed. They are the result of hard work and careful and devoted attention. And their value depends

on the insight and good judgment of their collectors. There are useless and misleading facts as well as useful and enlightening facts. There can be a great wastage of paper and of human effort in the publication of facts. Collections of facts for their own sake are of no more value than the collections of old objects.

Some facts are useful in themselves. Medical knowledge includes many facts about the effects of drugs and treatment for which there is no rationale, and as psychology extends into new fields it will welcome many facts about human behavior that cannot be fitted into any theory. The success of applied psychology will depend on the accumulation of much knowledge of this sort. And knowledge of this sort must be taught to students if they are to become practitioners. The open-mindedness of the physician toward facts of this sort which have no bearing on theories has saved the lives of many of us and is the real mark of the physician as distinguished from the scientist. The physician is interested in the cure of his patients. His success depends on his acquaintance with thousands of medical facts. The discovery of penicillin may lead to a saving of human life years that will match the loss of the fifty million lives in the war just finished. But until many researchers have patiently collected the relevant scientific facts that enable them to make scientific generalizations about how penicillin brings about its results, the discovery of its healing effect is not yet a contribution to the sciences of medicine and physiology. It is only a contribution to the tools of the physician.

My own personal bias in psychology is toward an understanding of learning and habit formation. In the two fields of learning and of motivation will be worked out the basic theory that will eventually make the science of psychology a much more powerful instrument than it now is. When we are able to state the general principles which govern human learning we shall have the most important tool needed for the prediction and control of human behavior.

Nothing is more familiar to men than human learning and habit formation. Men have lived intimately with the phenomenon since long before they reached the human state, and they have in myriad ways remarked and described it. But a scientific theory of learning has yet to be agreed upon by psychologists. Such a theory is essential to progress for several reasons. One of these is that unless the beads of fact can be strung in order and pattern on the threads of a theory, there is a strict limitation upon imparting psychological knowledge to others. Theories are mnemonic devices that make science teachable. And theories are the basis of working concepts. They enable men to confront new facts and deal with them successfully. Furthermore, theories are

required to direct the search for relevant facts. It is theories that endure, not facts. Events are ephemeral and their descriptions also may be ephemeral. It is theory that lasts for years or for generations. It is theory rather than fact that leads to new controls over nature and events. From theory inferences can be made and new applications devised. Facts are likely to be local and temporary. Their applications are limited.

A blacksmith may have collected much skill at his trade, and a few facts. He has learned that iron bought from a certain firm has certain good qualities. He can, by watching the color, judge the moment for making his weld. He knows how to temper his steel and how to draw its temper. The science of metallurgy goes in for a very different collection of facts. The science of metallurgy lies in making a different collection of facts. It is not interested in subjective color but in temperature. It substitutes chemical analysis for facts about market source. Market sources are ephemeral and smiths report colors less reliably than thermometers and thermocouples report temperature.

Like metallurgy, a scientific psychology consists in a new orientation toward psychological facts, a weeding out of subjective descriptions and an avoidance of descriptions colored by values and prejudices that are not universally shared. At the present moment our science is entirely too tolerant of such concepts as adjustment, reward and punishment, success and failure. These are all strongly flavored by values that are non factual. The approval of our own group or cult determines what we shall call an adjustment or what we shall call success.

It is my own conviction that in the field of learning the great majority of studies have been collecting unpromising kinds of facts. They have collected facts analogous to the blacksmith's lore concerning how long a particular tempering of his iron will wear upon the horse's hoof, how well pleased his patrons are with his wagon tires, how fatigued he will be with one method of welding as compared with another.

The reason for this is that we have allowed ourselves to be too much influenced by the desire for results of immediate practical application. This has led to the common acceptance by psychologists of a definition of learning in terms of practical value. Most psychologists, when they use the word *learning*, mean the acquisition of socially approved modes of behavior, improvement in performance, in economy of effort and of time in attaining conventional goals. The early writers on learning, Thorndike, Lloyd Morgan, Hobhouse, defined learning in terms of achievement. The animal learns a task set for him by the experimenter. He improves his accomplishment.

This conception, of course, is in good accord with practical common

sense. It is what gets done that is of practical importance, not the response of the person, but the results of that response. But to use practical achievement, goal attainment, success, as the essential criterion of learning, and to turn our search for facts to the observation of success and the conditions under which it is attained is analogous to the use of money value by the chemist as his chief descriptive term in observing a chemical reaction, or the definition by the physicist of work in terms of useful work or valuable work. All the psychologies which are written in terms of "least effort" or of goal achievement are by that choice rejecting the possibility of developing an objective and scientific psychology. They are, of course, following public interest which is turned toward securing quick results in training, or toward the abolishment of obnoxious habits, the acquisition of paying skills. We shall never learn how skills are acquired if we confine our attention to "improvement" in behavior and use as the criterion of learning the elimination of bad behavior and the acquisition of good, or the accomplishment of praiseworthy results. We must understand the processes through which behavior is changed, whether for better or for worse. A clinical psychologist is properly interested in the "cure" of an enuresis. If he is a real psychologist as well as a clinician, he will be interested in just what alteration in behavior was brought about; the fact that the alteration was acceptable to his patient's family may contribute to his income, but not to his science.

The conception of learning in terms of socially valuable outcomes of action led to the collection of learning curves which indicated the reduction of time and of waste motion with practice. It even led to a perversion of Pavlov's conditioning experiments in which as many as 1500 pairings of stimuli are recorded along with the resulting change in certainty or intensity of response. During that series the phenomenon of learning has occurred at each pairing. The massed effect of the 1500 trials may totally obscure or totally miss what happens at each trial. Studies with the maze, the puzzle box, the acquisition of skills, all record some end-result, but do not collect facts involving the animal itself.

The literature of mental tests had over twenty years ago collected some ten thousand titles and the number must be at least three times that figure by now. For the most part the testers have limited their collection of facts to the marks put on paper by persons being tested, and to the association of these marks with some criterion. They have not examined the behavior of the child taking the test, nor has this enormous literature advanced our understanding of what goes on in a child who marks the third of four possible choices. In other words, the

testing movement has been absorbed in highly useful and practical work, but it has not contributed to psychological theory. It has not advanced our knowledge of how the child's mind works.

In the same way studies in learning have been dominated by practical considerations and their facts collected center in practical outcomes of behavior rather than in the behavior process itself. We must undertake to examine the nature of changes in behavior before we shall have a proper understanding of success and failure.

My first suggestion for directing our attention toward facts that will lead to the development of good theory applies chiefly to the field of learning. It is that we look for facts in the behavior of the organism rather than in the operation of a latch, an arrival at a goal, the "learning" of a lesson. We should transfer our interest from the goal achievement to the behaving organism. It is the muscles of the organism that are innervated, and not the lever of the problem box. The machinery through which solutions are arrived at is contained within the skin of the solver.

May I illustrate what here is meant. Studies of maze learning have kept records of the time and number of errors required on successive trials to get the animal to a particular area. Learning curves have been plotted and learning assumed to be a direct function of the number of trials. Practically no experimenter has taken account of the fact that each animal may radically alter its behavior on successive trials, or that the alteration may have been evident only between the eleventh and the twelfth trials and exhibited no curve at all. The curve is only the resultant of many cumulative learnings, which may have included a number of "unlearnings" as well. The picture of learning as a function of the number of trials may be totally altered when we examine behavior at each choice point separately.

Dr. George P. Horton and I occupied ourselves two pre-war winters in observing and recording some eight hundred escapes of cats from a puzzle box. One startling result of an examination of our photographic records of the posture of the cat at the moment of release is the discovery that a series of escapes often displays a highly routinized pattern and stereotyped posture which appears at widely separated points in the series. Here is an elaborate series of movements extending over a period of many seconds or even minutes which has not disappeared from the cat's repertoire, although it has not been in evidence in the cat's behavior for many trials. It was not unlearned or forgotten, as is proved by its accurate reproduction.

If we had contented ourselves with a record of the time required to escape, we should have missed the real nature of the learning process.

So far as we can judge, improvement in the sense of time reduction consisted in the gradual elimination of movement routines that left the cat in the box. The successful act itself always appeared suddenly, either in the very first trial or in some subsequent trial. It required no long series of repetitions for its establishment.

It has been suggested that it will be profitable to give more attention to the behaving organism if we are to understand learning. There is a second admonition which might well be taken seriously. This is that we may profitably give more attention to stimuli as the occasions for response. No psychologist has seriously challenged the conception that the normal occasion for muscular contraction, and hence for all that an animal does, is the activation of sensory receptors. There are psychologists, however, who believe that the stimulus-response formula has had its day. For myself, I do not believe that it has been yet properly exploited. It requires that, if we are studying learning, we observe the response actually following stimulation. Many recent experimenters have, instead, followed Pavlov and observed not the sequence of stimulus-response but the conjunction of two stimuli like bell and food, or buzzer and shock, and have not believed it necessary to notice what actual response followed the signal.

Psychologists who think in terms of punishment and reward have almost uniformly neglected to note how the animal at the time responded to the punishment or to the reward, and the role this played in subsequent behavior. The resulting generalization is inevitably an attempt to link the intentions of the experimenter (intentions to reward or punish) with good or bad behavior on the part of the animal. Punishment and reward are, objectively viewed, stimuli acting on the animal's sense organs, and their effect must be mediated through the animal's nervous system and appear in muscular contraction or glandular secretion. Since levers and loops and mazes are not innervated, the operations of these devices are incidental to the actual learning which the living animal performs.

This failure to examine facts in the field of stimulus-response sequence is, of course, a tradition of psychology. Lloyd Morgan, Hobhouse, Thorndike, responsible for our first careful observations of learning, all were interested primarily in success rather than in response, and all speak in terms of "confirming results." It has occurred to none of them to regard these confirming results as possible stimuli, followed by possible response. Hull, who has endeavored to make the concept of reward over into something much more objective and immediate, so far as I can understand leaves the determination of what it is that will serve to confirm or reinforce quite vague. I believe it will be very

profitable to examine his reinforcements as possible stimuli with close attention to their subsequent responses.

There are many fields in psychology in which the injunction to note and formulate facts concerning the stimulus-response sequence might well be followed. Freud, who makes associative learning the foundation of his whole system, has at no point even asked what it is that is associated, or under what circumstances association is effective. A number of psychologists have in recent years insisted in interposing an O for "organism" between S for "stimulus" and R for "response." There can be no objection to this, provided we make a vigorous effort to determine the classes of fact that we agree to include in this O for "organism" and are not content to leave it as O. There can be no doubt that what is intended to be included in this O is often reducible to a dependence of response on interoceptive and proprioceptive stimuli, and O is a symbol for groups of relevant facts that should be noted and recorded rather than given up.

There is another more legitimate excuse for O. Very properly classed as characters of the organism affecting the stimulus-response sequence are the facts of past learning which can be known only through the record of past behavior. We have as yet no way of noting the brain changes that we assume to be the actualities responsible for changed response to stimulation. There are also legitimately included under O the determiners of behavior sought out by tests, which may be interpreted as behavior samples and assumed to be prognostic of response for varying periods of time. O may also include the material being offered through the more objective methods of examination and interview, and, by inference, the information furnished through the history of the individual.

In all these classes of fact it is of first importance to remember that facts are events so described that any competent observer will accept the description. We should recall that acceptance within limited groups, like the staff of a hospital working under an aggressive leader, or any department under an aggressive chief, may exhibit acceptance on grounds that do not insure that intelligent and informed outsiders will be able to agree to the asserted facts. Stimuli and movements are relatively objective and the agreement necessary to the establishment of fact is relatively easy to obtain. Attitudes and the meaning of behavior are less objective and more likely to produce disagreement among observers. Only on a factual level can the foundations of science be laid. Progress toward scientific psychology must be founded on agreed facts and public facts. The psychoanalysts' interpretations of dreams and of

motivation in general are notably remote from the factual basis that must precede the development of a scientific psychology in that field. But that a factual basis is not unattainable in a field so remote from basic theory as psychotherapy is established by Carl Rogers' recent book, *Counseling and Psychotherapy*, in that he has succeeded in reducing his account to descriptions of events which should prove to be acceptable to psychologists of varied interests and varied theoretical background. This he has even achieved in a number of his quantitative generalizations. He illustrates the type of fact collection which, though made with a highly practical aim, may furnish a basis for the theory that must eventually be developed in order to give system and order to our facts. That theory will extend far beyond psychotherapy into many fields of psychology.

My first suggestion concerning the factual basis for learning theory was that we give more attention to the organism itself, and that we recognize that such classes of fact as improvement, success and failure, reward and punishment, are external and incidental features of learning. The mechanism of learning is within the organism. These external features should be examined only in their role as stimuli to sense organs.

My second suggestion was that the promising factual field for observation is the stimulus-response sequence, and that we should meticulously note such sequences. My third suggestion was that part of what some writers insert in that stimulus-response formula, namely the organism, can, with diligence, be examined in terms of interoceptive and proprioceptive stimuli, often observable and often inferable (as in the case of so-called drives like hunger). Much of the rest of O names facts of the organism's past history, from which we infer changed tendencies to reaction. Such of O as is left over in the form of attitudes we must endeavor to place on a basis of public fact and seek for descriptions which are acceptable to all psychologists.

There is a further admonition. This is that we should undertake more consistently and thoroughly to note what I may call response-stimulus sequences, the stimulus changes following upon the responses of the organism. I have already expressed the opinion that learning and motivation represent the two fields most fundamental to an understanding of behavior and thought. Through close attention to stimulus-response sequences we may formulate the rules of learning, the circumstances under which such sequences change. Through close attention to response-stimulus sequences we may solve many of the problems of motivation and the direction of learning.

It is through observation of the effects of response on stimulation

that we may avoid those vague references to drive and motive that have done so much to obscure the understanding of behavior. A tense bladder through reflex paths operates to relax a sphincter muscle, but that relaxation is inhibited through associative learning by numerous situations. When these associative cues are removed or facilitating associative cues are added, the act occurs. The original stimulation is removed. The incident, save for its effects on future behavior through associative learning is for the time being closed. To invent a drive to explain this act is unnecessary as soon as we are familiar with the stimulus-response antecedents. To allow the disappearance of the restlessness that follows sphincter relaxation to force us to speak in terms of a drive that has attained its goal and is now satisfied is unnecessary when we observe the effects of the response on the new stimulus situation, the R-S sequence.

Every response alters the stimulus situation of an animal. Some responses remove the persistent and insistent stimulus that has been responsible for general activation as well as specific action tendencies. Such responses have a profound effect on the behavior following and on the mode of response that will be acquired by the animal through training.

Other responses leave the stimulus goad in action and the effect is to bring new goads into play. In fact the whole direction of behavior is set by the effects of responses on stimuli. The advocates of the law of effect (Thorndike) or the law of reinforcement (Hull) state the foregoing sentence differently. Their version would be: The whole direction of behavior is set by the effects of responses. You will recall that the version here suggested is: The whole direction of behavior is set by the effects of responses on stimuli. Punishment and reward have no effect on behavior as mere rewarders or reinforcers, but only in so far as they stimulate new behavior. We learn to do what punishment and rewards make us do. We do not necessarily learn to do what was rewarded or learn to abstain from what was punished.

In stimulus-responses there is to be found the key to associative learning. In response-stimulus sequences we may discover the motivation and direction of behavior. That we learn is insured by S-R. Stimulus patterns active at the time a response is initiated become inciters of that response. Because inciters of rival responses may also be active, the response does not always occur; but what effect such stimulus patterns contribute is toward the production of the response with which they were last associated.

That we learn is insured by the association of a stimulus with a re-

sponse. Whether that learning is retained depends on what then follows. It depends on the effect of the response on the new stimulus situation. May I illustrate this with an anecdote of animal learning. The anecdote is not factual in that it describes an event witnessed by only one psychologist and he would be too humane to repeat it. But its analogue is very familiar in the puzzle box behavior which George Horton and I have extensively photographed. The anecdote is this: The psychologist in question has a cat which on entering the kitchen before mealtime limps with a very noticeable limp. This limp is not observed in the cat at other times. Its history is that the cat on one occasion entering the kitchen at mealtime had its foot pinched in the swinging door. The cat made a terrific outcry and continued to limp about and put forth noise. After a quick examination to assure himself that no bones were broken, the psychologist offered the cat its dinner which had been standing ready. Why does the cat persist in limping on later visits to the kitchen?

Horton and I found that every cat we dealt with, between fifty and sixty in all, exhibited very similar behavior. When escape from the puzzle box followed almost any behavior, colliding with the release, pawing it, backing into it, jumping to the top of the box and falling on the release, lying down and inadvertently rolling to contact with the release, heavy odds could be placed that the same movement would be repeated soon after the cat was returned to the box. None of these behaviors had a learning curve. Each appeared suddenly full blown. In only a few cases could anything like an improvement of the successful act be recognized.

It is our belief that this characteristic of learning is explainable in terms of the effect of the response in question on the stimulus situation. Responses which left the cat in the box tended to disappear from its behavior, though in some cases they were very persistent. But the response which opened the escape door was generally preserved. We suggest that this is explainable through the fact that escape removes the cat from the puzzle box but does not allow a new response to be associated with the stimulus situations within the box. The cat has no way to forget. R remains faithful to its association with S because unfaithfulness would require that some rival response become associated with S, but S is now out of the picture. No new associations can be established with an absent stimulus situation.

It is my contention here that we shall gain much new light on behavior if we devote ourselves more zealously to observing the effects of response on stimulation. Every response must have such effects.

Through movement an animal changes its view, the sound pattern affecting its ears, its own pattern of proprioceptive stimulation from muscles and joints. In this radical change which results from action lies the explanation of the direction of our learning.

Our own responses not only bring about changes in the external world. They furnish cues for our further action. They eliminate, or sustain, or produce stimuli to action. And the consequences of this elimination, sustaining, or production are far-reaching.

My dog reaches out and paws my foot as I sit reading. To get my attention, the ordinary observer would say. Of course, the dog does it to get my attention. This is not a fact, however, but an interpretation. Its factual basis is that the dog makes movements or takes a posture that was in the past formed and originated by my attention. Without this factual basis we are speaking on the level of Little Red Riding Hood who is satisfied by the wolf's explanation that his great ears are the better to hear her with.

What would we find the explanation of the dog's gesture if we were to follow the rules that are here suggested and note the history of the event in terms of stimulus-response sequences and response-stimulus sequences? I reject immediately those softer accounts in terms of insight. There must have been a first use of the gesture, and I do not for one moment believe that dogs come into this world equipped with so strange a power for getting results. My own notion of what has happened is that first, I have on a number of occasions scratched the dog behind the ear. The effect of this on the dog is to interrupt all other activities and keep him motionless in place. Common speech uses the word enjoyment; but we might try to stick to psychological facts. The stimulation of my scratching is an essential element of the dog's response. It is what serves to maintain his quiet pose. When my scratching stops, the dog is released, but the sight of me, and the nature of his own response to me serve to call out in him a repetition or prolongation of his "behavior of being scratched." When scratching stops, he is no longer kept quiet by the scratching and is free to move. Whatever movement takes place will be within the limits of his present stance. I do not expect sudden barking or violent action. He has been standing quietly. He may move his head. If his movement had attracted my attention and brought a resumption of the scratching, I should expect that on the next occasion on which the stimulus situation was substantially the same, there would be a repetition of the head movement. This response of the dog's would not be unlearned, because the stimulus situation (waiting unscratched) which had become its cue with one repetition

is gone with the recurrence of the scratching. It is more or less of an accident that his original head movement was unrewarded, and that reward was reserved for a movement of his paw.

I have used the word "reward." That is not a psychological word. My scratching was not effective because it was a reward. It was effective because it prevented the dog from unlearning his gesture with his paw. If I had, instead, cuffed him hard, the cuff would have undone the gesture with the paw as a habit, not because the cuff is a punishment but, in psychological terms, because a hard blow would have established in the dog a tendency to back away from me and there would have been no recurrence of the situation that led to the gesture with the paw.

Once we have let ourselves in for scratching a dog's ears, there is no natural end to the incident save our own fatigue. The dog's response, his quiet posture permitting the scratching, can be continued indefinitely and will be interrupted only by eventual fatigue or an adventitious external event. But there are other actions that are self-terminating. Descending a stair cannot go on indefinitely even though the first steps have established associative serial connections, because one of the peculiarities of staircases is that they have a bottom step and it would be only with the help of a miraculous steamshovel and a corps of engineers that we could be kept provided with steps to descend. If we raise an arm, the arm is now raised and the situation radically changed. The arm can not be again raised until it has first been lowered.

This introduces a new admonition for our fact-collecting. Not only should we note S-R sequences. We should further note that any R in progress, whether that response is active movement or merely the maintenance of a posture, sets strict limits on what can next be done. At any moment there are severe restrictions on the behavior possible to elicit, no matter what new stimuli are offered.

We recall in this connection the work of Magnus on postural reflexes. When a cat has been decerebrated and is stood upon a surface, slight manipulation of its head can result in alterations of its whole muscular set. If the head is turned slightly to the right, the right fore-leg is flexed, the left extended, and the whole posture is made an obvious set for moving to the right. Older members of the profession can recall taking advantage of this postural adjustment by using leathern straps attached to the head of one of the larger animals in order to induce locomotion toward the side on which the rein was pulled.

It is probable that an intact cat in the posture appropriate to taking off toward the right, can not be directly stimulated to take off toward the left. The original posture must be first relinquished and a second taken

on. A whole field here calls for research. What movements are possible from a given stance? What responses are elicitable when a person is maintaining a given attitude? I would here include not only obvious physical attitudes but the more covert states which limit behavior. We must not give up the investigation of attention and readiness.

What are the effects on a going action of sudden irrelevant stimuli? I confess that I do not even know the answer to so simple a problem as what a diner in a restaurant, just raising his cup of coffee to his lips, will do if a police whistle is suddenly shrilled just behind his head. Will the movement in process be suddenly energized and the cup thrown over his shoulder, or will the cup be dropped from his fingers? We should know enough about the rudiments of behavior to answer such questions without waiting for some drunken guest to conduct the experiment for us.

Certain recent experiments in conditioning applied a signal under circumstances which allowed the animal either to have the leg flexed or to have it extended at the time the signal was given. There is small wonder that the results were ambiguous, since response was bound to be ambiguous. Extension is impossible when the leg is already extended. Flexion can occur as an active response only when the leg is not already flexed. This reminds us also of the original admonition to note the facts of stimulus-response sequence. When a signal is alternately or at random presented during extension and flexion or during running and during cowering, but no record is kept of the response following the signal, we should not expect to find any definite generalization in our returns.

Horton and I had a very considerable amount of fact-trouble in making our observations of our cats. Though we made notes and in a number of cases a motion picture record, there was often doubt whether or not a sequence of movements of the cat in the box could be reported as substantially the same as a previous sequence. The statement just made that the major determiner of the animal's actions is the present state of action or rest is an interpretation rather than a fact. It is an interpretation to which we found ourselves compelled; but it is not an interpretation to which we could be sure other psychologists would be forced. A large part of the time we could tell at any moment what this particular cat would do next. Our ability to do so was based on having seen it execute the same routine in a previous trial or earlier in the current trial. Having started any former routine, we could predict its continuance. It is possible that this prediction should have been undertaken, to compare with prediction on any other basis. This seemed, however, a bit absurd, since we knew no other basis on which to base

predictions. The behavior of other cats allowed such prediction only in the most general terms, and did not apply to specific movement series.

The response-stimulus sequence to which I have been referring includes, of course, the familiar concept of "set." What I am urging is that the development of basic psychological theory demands an extension of our collection of facts in this class. We should extend radically our knowledge of sets and their consequences, and we should do that by observing what responses are elicitable from a given stance or set and what are not. Social psychologists are collecting facts about attitudes and their patterns, particularly those organized about words. This must eventually be reinforced by further theory of the elementary behaviors out of which attitudes are made. The watchful therapist is full aware of attitudes in his patient. The skilled mental tester learns to direct a child's behavior into attitudes that permit testing. We must eventually know more of the facts of these attitudes. What is the factual description of negativism or of resistance? To what extent are such attitudes general in their effects and to what extent specific? About what cues are they organized? What occurs when an attitude of resistance changes to one of acceptance? How do prevailing attitudes control attention and perception and learning? In other words, what effect does our own behavior have on our own behavior?

I am reminded here of an extremely interesting paper of Heidebreder's on cognition recently published. In that she reports the results of some experiments which establish a certain hierarchy among concepts. Confronted with three sets of material in one of which the cognition is of an object, in another of a common form, and in the third, of a common number, the three are arrived at by subjects uniformly in that order—object, form, number. My own interest would be in the process through which cognition is attained. That process probably includes "trial and error" naming, and is not hopelessly un-get-at-able. My point is that cognition is not a mystery which involves a hidden thought process and a mental concept for which eventually a word is found, but is a case in which the subject uses his verbal repertoire in trial and error fashion until the word that "works" is hit upon. Proper search may discover that it is the careful noting of stimulus-response sequences that will furnish the factual basis for a theory of cognition. The cognition of objects may prove to be more ready than cognition of form or number because physical objects have a way of offering stable and recurrent, dependable, patterns of stimuli. Cognition of number may prove to be dependent on the initiation of counting which may be injected early in the process by an associative hint, or wait until trials

of object names prove futile. Concepts will turn out to be language in use, and use will turn out to follow the changes of learning by the same laws that will be worked out for action.

Up to this point a number of suggestions have been made for the direction of our search for psychological facts, for the ultimate purpose of understanding learning. One of these was that we have tended to neglect the behaving organism and to give undue attention to the external results of movement on the outer world. A second was that we would do well to recall that stimuli are the normal occasions for all response. A third was that we should note carefully the sequence of stimulus and response if we hope to get at the basic principles of associative learning. A fourth was that our interest in the role played by the organism in determining the response should be responsible for stern efforts to develop an objective, factual basis for our descriptions of the states of the organism that enter into the determination of behavior. A fifth suggestion was that closer attention to the response-stimulus sequence would be profitable in explaining motivation and the direction of learning.

My final concern is harder to name than these. I am in entire sympathy with the belief that quantitative treatment is to be aimed at in all scientific fact gathering. Number is the chief tool of science. But in our zeal to be scientific, I am convinced that we have been led into certain lines of experiment in the field of learning because these lines promised at least to yield numerical comparisons, curves. Because repeated trials can be given a series of ordinal numbers we have too readily fallen into the practice of treating the number of trials as a quantity, the more trials the better. We have been led to neglect what I am convinced is the central problem of learning, namely, what change occurs in behavior as the result of a single action.

In the laboratory we glory in experiments with fifty to fifteen hundred repetitions and their resulting curves. In nature these repetitions, as exactly duplicated as possible, simply do not occur. But learning does occur. The experimental results with a long series of repetitions have all the desirable characteristics of scientific fact. Their numerical analysis can be made by agreed methods. We must all recognize a mean and a standard deviation, or a difference and the standard deviation of a difference. And we are fairly agreed on the inferences that can be drawn from such analysis.

In the field of learning this very commendable effort to be scientific has led us toward studies of success, the trend of errors with repetition,

the reduction of time with practice. But it is a characteristic of a score of total errors (in a maze, for instance) to omit examination of the successive changes that constitute learning. Indefinite amounts of unobserved learning may enter into our final result. Before our criterion of success may be reached, learning, unlearning, relearning may have occurred over and over again.

We expect our friends to remember an engagement after one notice. We expect clinic patients to be different at each interview because of the last. We expect one quarrel to change attitudes. We expect one reading of a paper before this Association to leave some auditors with an impression. One spoiled egg may leave us for a time cautious. Once a rat has visited our grain sack we can plan on its return. Terror is called out once in the bird dog by the report of his master's gun, and the dog is now gun-shy.

But in the laboratory we assume that the response fixed in fifty trials was one fiftieth fixed in each trial.

Repetition has its place in learning, but repetition is effective only in those complicated instances in which what is learned is not a response to a stimulus, but a whole repertoire of responses to a large variety of stimuli. We have learned to achieve some result by means which vary according to the circumstances. Learning skills takes time and practice and furnishes beautiful learning curves and admirable data for statistical analysis. This is because they involve many and complicated learnings. It is here being suggested that the development of a scientific psychology requires that we investigate learning in its simplest forms. What happens as the result of one pairing of a stimulus pattern with a response that alters the previous effect of that pattern?

No group of psychologists has done more toward investigating this phenomenon in its elemental form than the Yale group under the inspiration of Clark Hull. With that work I have only one quarrel. This is that they have not examined adequately the R-S sequences which I have mentioned. Hull's theory, which has dominated the collection of facts in the Yale laboratory, is the theory of reinforcement, not a straight associationism. It assumes that an association is formed, or is not formed by virtue of a subsequent reinforcement or reward which somehow works upon traces of the S-R event and confirms or destroys the associative connection.

This theory is in line with the great tradition of the psychology of learning. Thorndike in his *Animal Intelligence*, C. Lloyd Morgan in his book of the same title, and Hobhouse in his *Mind in Evolution* all

speak in terms of a confirming reaction, which determines whether or not the association will be made.

No one questions the effectiveness of reward and punishment, or the effect of after-effects of a reaction on learning. But this statement of learning theory has led to an entire neglect of the observation of R-S. The confirmation or reward or punishment is supposed to have its effect by virtue of simply being confirmation or reward or punishment, not by virtue of the effect which it has on the stimulus situation and therefore on subsequent behavior and the opportunities for further learning. There is excellent reason for believing that both reward and punishment are effective by virtue of what they make the animal do, not simply by virtue of their own nature. Adherence to the theories of confirmation or reinforcement has led to quantitative results, it is true. It is highly probable that close examination of the action caused by punishment and the action caused by reward will discover that the learning which takes place can be adequately described in terms of the new associations set up by the new action. Reward, as Thorndike has remarked, tends to leave the animal doing the same thing in the same situation,—eating while food is present. Punishment induces the animal to do something different in the same situation. A theory of associative learning in its straight form without appeal to after-effects would lead us to predict in these instances what happens. The animal does not unlearn its tendency to do what it previously did if rewarded because nothing has happened to establish rival responses to the situation. It does not learn not to eat when the food is finally presented although it does eventually desist, because either the food or the inner hunger is now absent and cannot be re-conditioned in their absence.

Culler's laboratory, like Hull's, has led in the investigation of relevant facts on learning in its elemental form. Some of that work I should like to see repeated with closer adherence to the S-R prescription. Stimuli are applied without observing what the animal's actual next behavior is. In a number of instances two rival responses, flexion and extension, take place following the signal. That the result of this mixed practice turns out to be not a straight exemplification of association is not to be wondered at.

In general the experimenters who work with what has come to be called instrumental conditioning also fail to observe the maxim to observe the response following the stimulus. It would seem obvious that an investigation of association would require in the first place that the stimulus and the following response be at least made a matter

of record, but experiments in instrumental conditioning seldom record what the animal was doing when the signal was given. The returns therefore throw no light on association, but only on the effects of reward on subsequent response to the signal. In these experiments not only does the S-R fail to be observed, but also the sequence, R-S'. None of the experimenters is interested in the immediate behavioral consequences of reward, but only in the remote effects of reward upon a previous stimulus.

May I here recall the initial theme of this paper. It has been concerned with the future development of psychology as a science and particularly with the possible effects of a sudden increase in the numbers of psychologists and a sudden enormous extension of the application of psychology to practical affairs. None of us doubts that human living will be improved by that extension. Most of us would accept that improvement as the final goal and justification of all human science. But we must remember that the sciences have developed through an objective detachment from immediate profit, and that, in the overwhelming majority of instances, steps forward in scientific theory have been independent of practical application.

The hope that is here being expressed is that the new psychologists will in general not allow themselves to become mere technicians using psychological methods and techniques for the accomplishment of practical ends, that in the training of the new generation of psychologists we take care to cultivate an interest in theory as well as practice. We are entering a period of increased usefulness. It is to be hoped that it will not be a period in which theory stands still. Our factual information is bound to increase at a greatly accelerated rate. For that increase to result in the advance of psychology as a science two things are necessary. One is that theory be continuously produced and continuously amended and continuously used to guide the collection of fact. The other is that we remember to conform to the rules that have been responsible for the remarkable achievement of the scientific tradition, the use of objective evidence which means a basis in facts open to the observation of all who are interested and described in public terms that must be accepted by other scientists. These requirements may bear heavily on many current movements in psychology, in which recognition of events is claimed to be an art not communicable by ordinary means, open only to the inner members of a cult and closed to outsiders. Facts may accumulate without theory; but they will prove to be unstable and of little profit in the end. Theories may flourish if their basis lies not in scientific fact

but in opinions and interpretations acceptable only to the members of a limited faction; but they will be bad theories. Schools flourish only when theories are not carried back to public facts. Unless psychologists maintain an interest in general theory the fields of psychology will increasingly become independent collections of undigested information.

SHOCK THERAPY: PSYCHOLOGIC THEORY AND RESEARCH

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INTRODUCTION

For more than a decade contemporary psychiatry has been employing extensively as a treatment for psychiatric illness the procedure of artificially inducing comatose and convulsive states in mental patients.

It is not at all difficult to find historical antecedents in the care and management of the psychiatric patient which superficially, at least, appear to be like the current methods of shock therapy in that a sudden and sometimes quite aggressive change is effected in the subject's environment leading to a precipitous alteration in the patient's physical and psychophysiological state. The content of these methods varied with the change in cultural thought and are, indeed, as adequate symptomatic reflections of the history of psychologic thinking as, for example, are the historical changes in the interpretation of dreams or the evolutionary variations in the concept of the soul.

Occipital branding was perhaps one of the earliest of these methods, and the variations of the well-known water cures wherein the "frantic person was placed with his back to the water without being permitted to know what was going to be done" and who was then "knocked backwards into the water by a violent blow on the chest and tumbled about in a most unmerciful manner until fatigue had subdued the rage (159)" have a long history of therapeutic existence. Even in the time of Herman Boerhaave (1668-1738) "ducking" was a common psychiatric procedure as it was, too, in the remedial repertoire of Benjamin Rush, the father of American psychiatry. Additionally, Boerhaave recommended a special twirling stool on which by spinning a patient until he became unconscious his brain could be "rearranged" and the patient made normal (159).

The Roman, Cornelius Celsus (14 A.D.), in his *De re medica* wrote that a certain amount of physical coercion and a measure of gentle cruelty and the more refined tortures of inducing fear might bring the mental patient to his senses. Phillipe Pinel, the famous psychiatric emancipator, also believed in fright as an effective psychiatric remedy, and Johann Reil (1759-1813), while prescribing throwing patients into water and subjecting them to the firing of cannon, also was a protagonist of other forms of non-injurious torture (159).

Certain followers of Asclepiades, like Themison, approved of large doses of alcohol, antedating by two thousand years the therapeutic efforts of Kantoro-vitch, Constantinovitch (75), Berrington (12) and others who have relatively recently attempted to modify schizophrenic stupor by brandy taken orally or by alcohol given intravenously. These students of Asclepiades were also convinced of the merits of whipping the mental patient as were also the Anglo-Saxon priests described in *Bald's Leechbook* who believed that if one took the

skin of a porpoise and worked it into a whip and then would "swing the man therewith, soon he will be well. Amen."

Blood-letting, perhaps reaching its height as applied to psychiatric treatment under the Parisian, Édouard François Marie Bosquillon (1744–1816), was also widely practised over a considerable period of time and must undoubtedly have produced in some patients surgical shock if nothing else.

Following hard upon the heels of the creation of the Leyden jar, the first electrical condenser, by Pieter van Musschenbroek in 1746, Richard Lovett 10 years later claimed success in treating mental diseases with electricity. Arndt (7) in 1870 reported good results in depressions after electrical shocking, and other investigators, notably Huff (67), Haynes (67), and Allbutt (4), favorably inclined toward the psychiatric therapeutic use of electric current.

Cyanide, which among other effects depresses brain metabolism, had been used by Loevenhart (65) as recently as 20 years ago to obtain remissions in mental patients. Much earlier, hellebore had been extensively used, leading to the term "helleborism" as meaning a form of psychiatric treatment.

Hippocrates had in his times observed that organic disease supervening upon mental illness sometimes caused an abatement of psychotic symptoms, and remissions in psychiatric patients after surgical anesthesia, aborted suicide, fractures, and intercurrent disease are known to every psychiatrist who has had any extensive institutional experience.

The enumeration of these historical forerunners of contemporary shock treatment, however, does not assume any necessary continuity or similarity of motivation in the use of shock therapy, unless one may speculate upon the possible common presence of the less consciously defined motivation of aggression against the patient evoked in the physician by the inadequately met challenge of the etiological and therapeutic demands of the patient's illness, and one may perhaps also tentatively consider the incidental motivational importance of the residual in the therapist's personality of child-attitudes of the expectancy of the sudden, miraculous, magic resolution of puzzling barriers.

Shock therapy, whatever the ultimate evaluation of its therapeutic efficacy or desirability may be, provides extraordinary research opportunity under fairly controlled conditions for the investigator interested in either the psychologic or the neurophysiologic descriptions and interpretations of the multitude of happenings associated both with the active process of treatment and with the immediate and remote results of these therapeutic shock procedures.

For the psychologist, the controllable creation of convulsions in the human subject makes possible, for example, a precisely defined investigation of personality reintegration after convulsive dissolution. Many of the characteristics, both theoretical and actual, of psychological ontogenesis may be studied in this reintegration of the personality. Disturbances in perceptual organisation and the relation of such disturbances to general cognitive functioning, the description of the de-

terminants of perception, the relation of memory-content to problem-solving, learning, and logical thinking, the relation between emotion and memory and the relations between emotion and various personality dysfunctions, and the investigation of general problems of learning and habit-structure are but a few of the subjects which may be studied, from perhaps new aspects, by the observation of post-convulsive behavior.

The psychopathologist will be interested in what he may recognize, as did Sakel, as "activated symptoms" of a psychosis, or he may see symptoms suggestive of failure of inhibition and control and of the release of ontogenetically earlier modalities of behavior. So, too, the relationship of the changes in the person which are produced by convulsions to the characteristics of the pre-treatment psychotic personality merit closer scrutiny than has as yet been reported. The field of prognostic evaluation and psychological aid in the selection of candidates for successful therapeutic issue has been more widely studied. Systematic psychologic comparisons among convulsive and comatose reactions produced by the various precipitating agents and the examination of the similarity and difference of the after-effects of these induced convulsions both with reference to the varieties of artificial induction and to the immediate and remote sequelae of endogenous convulsions have not yet been adequately accomplished. Some of such comparative evaluation, however, is now much less important because of the empirical selection of electrically induced convulsions as the currently preferred treatment in most cases, with insulin shock now considered as possibly primarily valuable in certain kinds of schizophrenia. Psychologic assessment of what has been the nature of the personality dynamics in the cases of remission and recovery needs to be increasingly critical as a basis for the rationalisation of the therapy. The psychologic as well as the neurophysiologic interpretations of the therapeutic efficacy and mechanism of shock therapy must be substantiated progressively by objective research.

Neurophysiology is interested in the happenings which afford an opportunity to adduce facts confirming or depreciating theories of nervous system function and of the neurophysiologic correlates of behavior. Artificially induced convulsive reactions exhibit various degrees of neurophysiologic dysfunction and these shock-induced syndromes appear to be reversible. Critical attention directed to these conditions provides neurophysiology with a pathological analysis of the nervous system, as well as affords an opportunity to attend more closely to the neurophysiology of mental disease.

Since the literature directly and indirectly relating to shock ther-

apy has become so extensive, the formulation of future psychologic research and theory may be aided by a summary and critical review of pertinent psychologic description and interpretation evolving out of the widespread application in psychiatry of these recent therapeutic techniques.

SHOCK THERAPY

Induction Methods and Characteristic Reactions

Insulin. In 1928 Manfred Sakel (124) began to treat abnormal mental conditions by insulin-produced hypoglycemia after his experience in treating drug addicts for abstinence symptoms with insulin in order to depress the "dominatingly increased" activity of the sympathetic nervous system which he considered was associated with the withdrawal effects. In 1933 in the University of Vienna Psychiatric Clinic he began to treat schizophrenics by induced hypoglycemic reactions. However, it had already been discovered in Oslo (83) quite by accident that a mental patient could be "cured" by the induction of hypoglycemia, and Day and River had independently made the same discovery here in America. An Hungarian doctor also claimed to be using insulin in the treatment of mental conditions a few years before Sakel initiated his treatment.

Sakel's original procedure of treatment has undergone many modifications, but, in general, sufficient insulin is given to induce first quietness, then drowsiness, sleep, and finally coma. A comatose reaction has ensued in an individual with as little as five units of insulin and has not followed in another patient even after 400 units of insulin. The variability of individual behavior in the hypoglycemic reactions is considerable. Some patients evidence great restlessness and even excitement before going into coma, while others may attain the comatose state without much expressive activity at all. From the physiological aspect, sweating, increase in pulse rate, and muscular movements may be seen in the pre-coma. Isolated twitching and jerks in the body musculature may then merge into a generalised convulsion, although usually the coma is interrupted before a convulsion eventuates.

Sakel writes of the cerebral functions gradually disappearing, level by level, as the hypoglycemia intensifies and then returning as the patient awakens within the quarter hour after termination of the hypoglycemic reaction by the administration of glucose. Frostig (42) has further detailed the neurologic description during five hours of the hypoglycemic process. In the recovery period confusion, psychomotor excitement, dysphasia and other disturbances are seen. Most patients are amnesic for the period of hypoglycemia and where the coma has been

prolonged or where a convulsion has occurred retrograde amnesia is observed. In this latter respect it is interesting to know that Sakel in his early studies attached a certain therapeutic importance to epileptic and epileptoid motor reactions and stressed the necessity of long treatment not only for the modification of secondary symptoms but also for the control of the symptoms of the "activated psychosis" which Sakel thought to occur at a stage in the treatment when the patient showed no signs of his psychosis except during the hypoglycemic state. However, as Kalinowsky (70) particularly points out, the acute psychotic picture which Sakel called the activation of the psychosis is undoubtedly a transient organic reaction to the treatment and very probably would occur in a non-psychotic subject as it does, indeed, in psychoneurotic subjects.

Early in the development of insulin shock therapy the Viennese school stressed the importance of a period of euphoria, accessibility, and inhibitory release in the early hypoglycemic period, although the occurrence of such behavior is by no means a constant feature of insulin shock induction. Psychotherapy was used during the existence of this condition as well as in the post-coma phase of recovery. Such transitory insulin-induced accessibility may be compared to current narcoanalytic technics using soluble barbiturates. Bleckwenn (15) had in 1930 used sodium amytal in the treatment of the psychoses. In connection with the mention of the narcoanalytic technic it is interesting to observe that Thomas (143) and others have reported that mute and inaccessible psychotic patients who respond to intravenous sodium amytal will also respond to shock therapy and Thomas has proposed the use of sodium amytal as a prognostic aid in predicting the outcome of convulsive therapy.

There is no space in this paper to discuss the possible neurophysiological mechanisms of the psychosis-modifying effect of insulin treatment. Sakel (125) thought that adrenalin hyperactivity produced a lowering of neural thresholds in the cerebrum, thus reviving the phylogenetically oldest and infantile constellations and pathways among the neurones. Insulin neutralises the effects of adrenalin and was thus considered to reverse the whole process, the resulting hypoglycemia producing a vagotonia. Gellhorn (44) believes that insulin leads to an excitation of the sympathetic-adrenal apparatus through hypoglycemia of the brain, and Heilbrunn's (58) experiments are in favor of the hypothesis that insulin creates a sympathicotonia, although Hadorn (56) has indicated that such effects may depend upon the dosage and he has demonstrated that small doses of insulin stimulate the vagus while large doses cause a primary secretion of adrenalin. Pfister (58) thought

with Sakel that hypoglycemia damps the sympathetic nervous system.

However, as Riess and Berman (117) have indicated, there is no evidence of adrenalin overproductivity in schizophrenia or in other psychotic states. Insulin, in fact, increases adrenalin in the hypoglycemic phase. Parker (108), too, has shown that the autonomic functions during the hypoglycemic states are marked by oscillation between sympathetic and parasympathetic tonus, due to the increased activity of one system rather than a diminished tone of the other. Moreover, this investigator found that recovery occurred both in cases where chiefly sympathetic and where primarily parasympathetic excitation was evident. Fortuyn (37) also observed both sympathetic and parasympathetic activity in the hypoglycemic state.

Other explanations have stressed, like that of Georgi (45), the decrease of permeability of neuronal membranes in the psychoses facilitating the retention of toxic products, a condition which is then altered by the insulin effects. Assumptions have been made, like that of Demole (28), which are based upon a belief that insulin tends to correct disturbances of neuronal oxidation which results in an accumulation of sub-oxidised products of metabolism which produce schizophrenic symptoms as a form of cerebral toxemia. Unfortunately, these descriptions point to an insulin effect upon an assumed but unproven physiopathology of the psychoses.

Metrazol. This is a synthetic compound clinically related to camphor. In this substance the six carbon ketone ring of camphor is condensed to form pentamethylenetetrazol (53). Although Paracelsus had used camphor as early as the 16th century to produce convulsions and although Oliver in 1781 treated a manic psychosis by camphor-induced convulsions, cardiazol, as the synthetic compound is called in England and on the continent, was synthesized and first investigated pharmacologically by Schmidt, Hildebrandt, and Krehl (127), and it was soon being used as a stimulant in circulatory and respiratory collapse. In 1926 Blume (16) found it possible with large doses to produce convulsions in animals.

Meanwhile, a few years before the use of psychiatric convulsive therapy was begun, Nyiro and Jablonsky had observed in 1929 that cases of epilepsy in which schizophrenic features were prominent recovered rapidly from the schizophrenia when fits were frequent, and in 1930 Müller (97) described two cases in which schizophrenic illness recovered quickly after the appearance of spontaneous epileptic fits. Glaus (49) had also reported a year later that the combination of schizophrenia and epilepsy was rare, although such an observation was certainly contra-

dictory to textbook descriptions of the well-known continental psychiatrists of the time, like Kraepelin and Bleuler, who found nothing extraordinary about the diagnosis of schizophrenia with epilepsy. However, in contrast, it is interesting to note that Steiner and Strauss, writing in Bumke's well-known *Handbuch der Geisteskrankheiten*, would question the correctness of the diagnosis of schizophrenia in any case with convulsive attacks.

Glaus investigated 6,000 schizophrenes and found that only eight had had fits at any time in their lives and, more interestingly, he discovered that four of these eight patients recovered shortly after the fits appeared. Other scattered reports in the literature also indicated a possible antagonism between schizophrenic catatonia, at least, and epilepsy.

In 1934 von Meduna, the director of a mental hospital in Budapest, who had been experimenting with camphor-induced convulsions in animals, began to produce convulsions in chronic schizophrenics by the intramuscular injection of 25% camphor in olive oil, being motivated largely by the consideration of the possible salutary effect of epileptic seizures on the schizophrenic illness. In addition to what he considered the incompatible occurrence of epilepsy and schizophrenia in the same individual, Meduna also sought to establish that epileptics belong chiefly to Kretschmer's athletic type and that the neuroglia in epileptics is hyperplastic in contrast to the supposed neuroglial aplasia of the schizophrenic. The carbohydrate metabolism was also evidenced to be slowed in schizophrenia and accelerated in epilepsy.

Against such evidence of the antagonism of schizophrenia and epilepsy there have been several observations published in recent years. The paper of Yde, Lohse, and Faurbye (158), for example, recounted the data of 715 cases of schizophrenia 20 of whom had had convulsive attacks, and epilepsy was established in five of these cases, which is about twice the incidence expected by chance. Hoch (66) has also indicated a positive tendency for epilepsy and schizophrenia to be associated. Moreover, the standard classification of mental disease, in this country at least, may lead to serious statistical collection errors in that schizophrenic symptoms occurring with epilepsy are very likely to bear the diagnostic label "Psychosis due to convulsive disorder (epilepsy)" and so completely escape the attention of the searcher who looks only for schizophrenia with an accompanying epilepsy. As this standard nomenclature indicates, the organic bias of contemporary institutional psychiatry will probably put the major diagnostic emphasis upon the epilepsy.

Most metrazol convulsions are now induced by the intravenous injection of the drug so that the usual interval between the commencement of the injection and the initial signs of convulsion is about ten seconds. The reaction may be either subconvulsive or generalised and is usually evidenced by sudden pallor, a short cough, muscular twitching, and a terrified expression, and sometimes by a cry or scream. Unconsciousness after the fit lasts for variable intervals and the post-convulsive recovery is characterised by the same sequence of events as occurs after any generalised convulsion.

Intramuscular injections of triazol produce a slower prodromal convulsive picture than intravenous metrazol and considerable expressive activity may be seen in the patient before the convulsion is precipitated.

Multiple convulsions may be produced by large doses of cardiazol.

Denyssen and Watterson (29) believe that the convulsion following cardiazol injections is due to sudden vasoconstriction, but Georgi (46) thinks that the pathogenic factor in the initiation of convulsion is not to be found in a primary vasospasm but in the ionic change at the cell membranes, and he has suggestively demonstrated that the ionic changes occur only when the cardiazol injection is sufficient to produce a generalised convulsion. This is possibly significant in that it is generally accepted that subconvulsive reactions are therapeutically much less effective than are total convulsive reactions, although the contention of Polatin, Spotnitz, and Wiesel (116) that in the insulin procedure hypoglycemia alone without coma or convulsion is adequate to bring about remissions must be kept in mind. This observation is also substantiated by Krasnouchkin and Hanlarian (82) who obtained very favorable therapeutic results both with and without provoking reactions of coma. Hill (64), too, has claimed success by treating schizophrenia and other mental diseases with small doses of insulin (5-10 units) and histamine. Hill proceeds on the assumption that there exists in psychiatric disease a pathologic barrier in the endothelial walls of the capillaries against the normal interchange of plasma. Histamine, particularly, is considered to affect this pathologic barrier so that capillary walls are made more permeable to the osmotic cell-nutrient interchange.

Ammonium Chloride. Bertolani (118) in 1938 began producing epileptiform seizures by the intravenous injection of a solution of ammonium chloride. This method has been very little used in the United States, but it enjoyed a temporary popularity in India in the hands of Rizvi (118) and on the continent and in England it had a number of protagonists, including Mazza (118), Dax (26), Martinez and De la

Vega (89), and others. The reaction usually consists of a pre-convulsive period lasting 10–20 seconds in which the face becomes flushed, and in which hyperpnea, pupillary dilatation and loss of reaction to light, and absence of corneal reflex are seen. The convulsive phase proper is usually mild but may be typically epileptic, although ordinarily only muscular twitching and extension and flexion of the legs and arms are observed. No cyanosis, incontinence, or biting of tongue is commonly present. Afterward the patient may be disoriented. It is assumed by Rizvi that the convulsive reaction is produced not by alteration of the blood pH but by the irritant effect of the ammonium chloride on the blood vessels, causing vasoconstriction.

Coramine. 12 to 15 intravenous injections of 10–25 cc. of coramine (Nikethamide) solution, a drug which like metrazol is a central nervous system stimulant, have been used as a course of treatment for psychiatric syndromes. Following injection, an initial apnea results followed by hyperpnea, accelerated heart rate, increased blood pressure, profuse sweating, tremors, and nystagmoid ocular movements. Consciousness is retained, but confusion, disturbance of attention, and general "clouding" of mental function may last for about 10 minutes. Anxiety and fear expressions are seen. Skorodin (131) claims 19% remission obtained in selected cases.

Electric Shock. The currently most widely used shock procedure originated from the use of electricity in producing convulsions in dogs. Leduc had in 1900 produced electrical sleep in animals, and Robnovitch (119), working in Leduc's laboratory, found that her electrical stimulation of the dog's brain sometimes produced epileptiform convulsions, an occurrence which five years later was confirmed by Weiss (152). In 1929 Viale (145) was inducing epileptic seizures in dogs by placing electrodes in the mouth and rectum and passing an electrical current. Cerletti (21), who had also had extensive experience in the production of electrically induced convulsions in animals, inaugurated in 1938 with his collaborator, Bini, the electroshock treatment of the human psychotic subject.

Four hundred to 500 milliamperes of alternating current are usually passed for about .2 second through electrodes placed on the fronto-temporal cranium. Subconvulsive reactions or generalised convulsions ensue concomitantly with the passage of the current. Wilcox (153) conveniently divides the electroshock convulsion syndrome into an initial tonic phase followed by a clonic phase, an atonic phase, a stuporous phase, and a post-convulsive mental state.

The voltage of alternating current necessary to produce an effective

stimulus is usually more than 100 volts. Cortically initiated muscular movements may be evoked by less than 10 milliamperes when the stimulation is applied directly to the brain, but in electroshock procedures the intervening skull and tissues as well as the reduction of current density because of diffusion of current lines of flow make a relatively high voltage necessary. Also as Hemphill and Grey Walter (61) point out, the alternating current stimulus is repetitive. Since electrophysiologists believe that the physiologically important stimuli arise at the negative pole of the applied current, an adequate electrical tension applied for .2 second in 60 cycles of alternating current will result in 12 effective stimulations of each cerebral hemisphere. The duration of current application, therefore, is the moderator of the number of times the brain is electrically stimulated. The smaller the number of current volleys, the greater is the current strength necessary to induce convulsion. It has also been shown by a number of workers that Ohm's law does not apply to the passage of current through tissues. With the flow of current the initial resistance is modified by the factor of reactance and possibly by capacitance and other determinants so that the measurement of resistance is of little value in calculating current effect.

Friedman and Wilcox (41) have used unidirectional wave forms of current, including half-sine waves, galvanic current pulses, combinations of these two forms, and repetitive condenser discharges. It has been stated that some alterations in the wave form of the current produce the convulsive effect with less amperage and lessen the post-convulsive disorientation and dysmnesia.

Berkwitz (10, 11) introduced an electrical shock method in which the stimulation is effected by about 30 shocks of faradic current from an induction coil, one half second in duration, with current strength under the convulsive level. Two to 45 such treatments are given in which fear and some pain are characteristic reactions. He reports in one study (11) that 35% of chronic patients so treated showed improvement, and one complete remission was obtained.

Electronarcosis. Reminiscent of Leduc's electrically induced sleep, the introduction of an electronic apparatus delivering 160–250 milliamperes of 60 cycle alternating current continuously in the human subject for an initial 30 seconds through bitemporally placed electrodes has been a recent modification of electroshock therapy. The first 30 seconds of stimulation result in a tonic spasm with a few seconds of cardiac arrest and a 30–45 second respiratory suspension. The current is then dropped to 60–70 milliamperes, whereupon a few mild clonic

movements may be seen, and then after 60–75 seconds from the initial application the current is increased by a five milliamperere increment every 15 seconds to a maximum of 125 milliamperes at five minutes. The major differences from electroshock reactions are seen in a prolongation of stimulation of the autonomic nervous system, in prolonged flexor tone, and in the presence of forced grasping. These kinds of reaction are seen, however, in deep insulin coma, and Thompson (144) and his co-workers believe that electronarcosis is similar in therapeutic efficacy to insulin treatment and that it is superior to electroshock in the management of schizophrenia.

Other Methods. The employment of picrotoxin, anaphylactic shock, nitrogen anoxia, carbon dioxide stimulation, refrigeration, sterile meningitis, and a few other methods which have been used experimentally in an effort to modify human psychoses need not be discussed since they have not been used extensively enough to have provided any psychological data relevant to the interests of this review. Psychic "shock" is also being advocated by at least one current writer (35).

Prolonged Narcosis. Continuous narcosis of several days' duration is used most frequently in contemporary psychiatry in the handling of manic excitement and in the treatment of certain battle reactions. We must go back at least as far as MacLeod (62) for historical priority in the use of *Dauerschlaf* in the treatment of mental disease, although Locock in 1857 had advocated the use of bromide in the treatment of epilepsy and in neuroses, particularly hysteria. MacLeod, working in Shanghai in 1897, began the treatment of acute mania with bromide-induced narcosis of several days' duration. Previously, of course, the ancients had made conflicting observations upon the value of sustained sleep in mental disease, and in the middle nineteenth century Andral and van Swieten among others had seen maniacal conditions cured by accidentally large doses of opium. In 1901 Wolff (62) treated confusional states with trional, and Epifanio (62) in 1915 was using luminal. Kläse was encouraged by the results of somnifaine narcosis which he had induced in 26 schizophrenics and which he reported in 1922.

Prolonged narcosis is not, strictly speaking, a "shock" therapy, but a brief consideration is necessary because earlier it was used for all kinds of psychiatric disorder and reports, for example, like that of Palmer (106) who in a mixed series of cases treated with somnifaine obtained 33% recoveries are psychologically significant in evaluating the specificity of the nature of the shock-induced remissions. Meerloo (93) also obtained one-third recoveries in 500 well-mixed cases of psychiatric

disorder following prolonged barbiturate narcosis. Hennelly (62) breaks down his remissions to 42% in mania, 32% in melancholia (manic-depressive-depressed), and 44% in involutional melancholia.

Treatment Combinations. Early in the use of insulin, the hypoglycemic coma was interrupted by metrazol convulsions in order to obtain the therapeutic effect of the epileptiform seizures. Electric shock has also been used in conjunction with insulin. Moreover, failures with one procedure have been subjected to other kinds of shock therapy. There are psychiatric patients even who have initially undergone insulin shock, then a series of electroshock treatments, and who have then finally been subjected to frontal leucotomy. One of Freeman's and Watts' patients, quite interestingly, passed through a psychoanalysis, an insulin shock treatment, and two frontal leucotomies, and she is currently engaged in working on her doctorate thesis (39).

Adequate statistics and careful definition of the characteristics of pre- and post-treatment personality in these cases where more than one shock procedure has been used would be most helpful in the evaluation of the effects of these different shock methods. However, the literature is rather disappointing in this respect. Patients who have not responded to one kind of shock procedure may occasionally benefit by the application of another convulsion- or coma-inducing method. However, where such a change in the treatment technic is made with favorable results, this successful issue may in many cases very probably be due to the total length of the combined treatment and not to the change in procedures, since some patients may not show adequate modification of the psychotic symptoms until after they have experienced 20 or more convulsions. There is some indication that insulin is more effective than other methods in the treatment of schizophrenia, and hence it may very well occur that some of such cases may show a disappointing reaction to metrazol or electricity and yet improve as a result of the hypoglycemic regime.

More adequate research substantiation is needed before the superiority of any one shock procedure is established for the treatment of any specific psychiatric syndrome. More and more it appears that the intensity of treatment, of whatever kind, is the important consideration, and it may very well be, as Kalinowsky (70) suggests, that even manic excitement may be controlled by shock therapy provided the confusional form of treatment described by Löwenbach (86) is used, although Löwenbach, himself, believes that convulsive treatment is of little value in mania.

Illustrative of encouraging results obtained by a change of method is the report of Weinberg and Goldstein (151) who observed that 32% of 100 patients treated with insulin following failure to respond to metrazol showed good improvement.

Combination with Other Drugs. Various substances, such as curare, beta-erythroidin hydrochloride, or quinine methochloride may be combined with the convulsive agent in order to lessen the motor severity of the convulsion. Shock treatment has also been given with the patient under general and spinal anesthesia. Sodium amytal may be used to aid in the control of the resistive and apprehensive patient and in order to forestall post-convulsive excitement. Scopolamine has been similarly used. Cocaine has been employed to induce a pleasant post-convulsive emotional state. Magnesium sulfate has been combined with convulsion-evoking agents, and strychnine has been used to increase the sensitivity of the nervous system to metrazol.

Combination with Psychotherapy. Considerable controversy exists among psychiatrists as to whether shock treatment should be combined with active psychotherapy. In the case of any formal analytic process demanding continuity of progress and of the experience of therapeutic "movement," this problem is solved rather incontrovertibly in most cases by the inability of the patient to remember either the analyst or the content of the analytic sessions from one treatment day to the next. However, in many places where restrictions on time and personnel are not too drastic pragmatic psychotherapy is usually attempted. Even if the patient has amnesia for the period during which psychotherapeutic relations could be established, such interpersonal experience may quite conceivably be of considerable therapeutic importance even if it is not immediately reflected in prognosis. We must be aware, however, that Myerson (98), for example, allowed some of his ambulatory patients to be treated by electroshock with minimal personal contact, and he reported as favorable results with this group as with patients additionally aided by psychotherapy. Nevertheless, Orenstein and Schilder (102) pointed out some time ago that there is a difference in the kind of insight socially recovered patients may have, depending upon whether they have become aware of the psychodynamics of the self through the process of psychotherapy or whether they simply know, following successful shock therapy alone, that they were once ill and are now, somehow, well again.

The failure of later investigators using the insulin technic to achieve the good initial results reported by Sakel may be due in part to differ-

ences in the intensity and quality of psychotherapy employed by the different clinics. Sakel himself was willing to attribute 20–30% of the total insulin-induced remission percentage to the concomitant use of psychotherapy.

Convulsive therapy, because of the post-convulsive accessibility and helplessness of the patient, may provide the first opportunities for the establishment of psychotherapeutic relationships with a mute, inaccessible, or negativistic subject. Moriarity (65) believes also that the combination of convulsive therapy with psychotherapy yields better results than psychotherapy alone in treating the neuroses and the borderline psychoses. This observation, however, must be tempered by the consideration of the general agreement of opinion that shock therapy alone is of little or no value in the treatment of the neuroses, with the possible exception of the reactive and anxiety depressions and some of the more blatant hysterias.

Duration of Treatment. Insulin coma is usually induced six days weekly for at least one month so that as many as 30 to 40 single treatments may be administered and in some cases, many more. Metrazol and electricity were used generally to induce convulsions about two or three times weekly to a total number of 10 to 15. The emphasis soon came to be placed upon an adequate number of convulsions (at least 20) and upon a more frequent occurrence so that currently several electroshock convulsions may be administered even in the same day. Löwenbach (86), for instance, has stressed the necessity of achieving long-lasting disorientation in psychotic subjects by frequent convulsions as one requisite for successful therapeutic effect. Freeman (39), also, considers frequent convulsions and the resulting enduring disorientation desirable. However, the decision about the spacing of convulsions depends upon what the effect upon the patient of shock treatment is thought to be as well as upon the personality reaction being treated.

OBSERVATIONS OF IMMEDIATE TREATMENT REACTIONS

General Clinical Description. The actual behavior of patients reacting to convulsion-precipitating agents has been described with considerable agreement by many observers. Löwenbach and Stainbrook (87) have stated that a generalised convulsion results immediately in a state in which it is impossible to demonstrate operationally any behavior subsumed under the psychological conception of personality. "The individual does not react to any kind of stimuli and the activity of the electroencephalogram has almost ceased." The convulsion is then succeeded by the gradual return and reintegration of personality functions.

According to these writers, "it seems as if the subconvulsive reaction does not differ from the convulsive except in degree, but in these minor reactions . . . reintegration may begin at any level and the recovery may be much more quickly and indeed sometimes imperceptibly achieved." Cohen (23), studying the return of cognitive function following metrazol convulsion believes that the continuum of recovery can be rather adequately defined in terms of the initial and immediate post-convulsive anesthesia, aprosexia, agnosia, apraxia, and amnesia. After four minutes usually, tactile, visual, and other sensations begin to show evidence of function, and there then follows in roughly chronological sequence but with much overlapping, of course, and with progressively decreasing exhibition of dysfunction, the recovery of attention, gnosis, praxis, and memory. Stainbrook (137) writes that the cognitive behavior observed clinically following electrically induced convulsions may be considered conveniently as consisting of immediate transitory effects most economically conceived as reflections of severe neurological dysfunction and of a more remote and relatively enduring symptomatology of disorientation and dysmnnesia.

Ismael's (69) observations on patients during hypoglycemic induction recorded anomalies in sensory perception, including hypnagogic visions and hallucinatory images which were primarily visual but which occurred in all sensory spheres. He also saw what he inferred to be anxiety, terror, euphoria, rage, and panic. Misidentification of persons existed as well as disordered gesticulation and dyslalia. Evidence of exteriorized sexuality in the forms of homosexuality, sadism, and exhibitionism was also observed. Like von Angyal (5), Benedek (9), Palisa (105), Plattner (114), and others, Ismael was also able to demonstrate disturbances of the body-image. These latter investigators found that the hypoglycemic state produced changes in the perception of space and of color qualities and disturbances in gestalt comprehension and in concept formation. Pisk (113) described the appearance of the "Zeitraffer" phenomenon in insulin coma and reported a patient who experienced acceleration in the flow of time. Orenstein and Schilder (102) also report alterations of time perception occurring during insulin reaction. Benedek (9), quite interestingly, detailed disturbances in the perception of movement which were similar to those observed by Pötzl and Goldstein and Gelb in occipital lobe organic lesions. Benedek also reports that micropsia, megalopsia, and primitive optic hallucinations appeared and that colors appeared less impressive to patients during shock and looked more saturated after shock was over.

Silbermann (130), from his studies on subjects treated with insulin

and triazol, describes two groups of symptoms, one group being classified as regressive and occurring in the interval between the injection and the onset of the coma or convulsion and one group being seen after "shock" and described as restitutive. Among the regressive symptoms he would place feelings of giddiness and ill-defined feelings of apprehension, sensations of hot flushes, excitability, disturbances in the evaluation of shape, distance and size, abnormal sensations of color, distortions of auditory impressions, feelings of unreality, guilt and fear of punishment, confusion, loneliness, feelings of world-destruction, and fear of death. The immediate post-shock, or restitutive, group of symptoms includes, characteristically, feelings of fear, confusion, and unreality as well as motor and sensory aphasia, euphoria, and a sense of being like a helpless child. Good (51) has also described the pre-convulsive aura following metrazol injection as evidencing apprehension, perplexity, strangeness, and choking sensations and as culminating in intense fear and terror. Friedman (40) in the same pre-convulsive reactions saw "rapidly mounting anxiety which in certain cases rose to a vivid deliriform panic-state with concomitant characteristic vasomotor and psychomotor overactivity." Collins (24), too, stresses the emotional responses of fear and apprehension in the aura of metrazol convulsions and in the hypoglycemic pre-coma, and Hemphill (60) writes that fear of death may be consciously expressed by metrazol-treated patients. On the other hand, Cook (25), using a scale of the degree of fear exhibited by cardiazol and triazol treated patients as rated by attending nurses, concluded that the intensity of fear in 275 cases as recorded and correlated with the outcome of treatment offered no evidence for assuming that fear exerts any curative influence.

Interestingly, Dyne and Tod (30) in a comparative study of reactions to subconvulsive and convulsive doses of triazol found that non-schizophrenic mental patients reacted more prominently with fear and anxiety than did a group of "emotionally deteriorated" schizophrenics. Good (51), however, discovered that both psychoneurotic and psychotic patients showed the same fear, and he also saw no differences between these groups in their immediate post-convulsive behavior. He noted, though, that the post-convulsive disorder of dysmnnesia and disorientation was of much shorter duration in psychoneurotics than in psychotics, and this observation supported his contention that shortness of post-convulsive confusion and disorientation is a favorable prognostic sign. Löwenbach and Stainbrook (87) have put forth the opposite theory that a long post-shock disorientation after single convulsions is a good prognostic index.

Hemphill (60) postulates three psychological events in the course of convulsive treatment: (1) the realization of being treated, (2) the return to reality from the death-like state of the epileptic fit, and (3) the encounter with the environment after reestablishment of consciousness. The observation is also made that the patient is able to identify and name parts of his own body before objects in the external world, and it is therefore inferred that self-interest or awareness of the ego returns first in post-convulsive recovery. Opposed to this observation is that of Schilder (126) who states that, after metrazol convulsions, patients are able to name objects before they can designate correctly parts of their bodies. Additionally, Hemphill remarked that the use of certain emotionally descriptive adjectives may be seen as evidence of exaggerated affective attitudes existing in the immediate post-shock state. The post-convulsive subject is also described as feeling dependent and attached to persons in the environment and as assuming infantile attitudes.

Kalinowsky and Kennedy (73) have observed that post-convulsive phenomena following electric shock are surprisingly constant in each individual and they believe that convulsive and post-convulsive behavior follow the predetermined pattern of the individual and that these reactions are not influenced by the type of stimulus nor by previous medication.

Psychoanalytic Description. Rankine Good (51) divides the immediate post-convulsive state into four stages reminiscent of the analytic conception of sexual genesis: (1) the primary narcissistic stage, (2) the oral phase, marked by retention of gag, sucking and chewing movements, and spitting, (3) the anal phase, in which fecal smearing and coprophagia may occur, and (4) the phallic stage, during which fingering the genitals, clawing the vulva, exhibitionism, beating the genitals, and masturbation may be seen. It must be remembered, however, that these activities do not occur in every patient and that some of them are seen while the patient is operationally unconscious. Possibly as Löwenbach and Stainbrook (87) have suggested, some of these movements like sucking and chewing may be psychologically meaningless and may point to a cortical area being resistant to convulsive exhaustion. Mayer-Gross (90), discussing these oral and facial movements from a neurological point of view, suggests also that such movements may be motivated by hunger, at least in hypoglycemia. However, since sucking and chewing behavior may be seen after electroshock as well, it is unlikely that hunger is the explanation. Interestingly, Larkin (83) feels that he can prevent delayed insulin coma by stimulating oral sucking and by

"mothering" the patient by embracing and stroking him after glucose has been given to terminate the hypoglycemia. Abse (3) sees a confirmation of psychoanalytic theory in his challenging but inadequately demonstrated observation that, post-convulsively, manic-depressive patients show predominantly oral activities, while paranoid schizophrenes express primarily anal interests, and degenerative hysterics display mostly phallic behavior.

Specific Description. Orenstein and Schilder (102), and Schilder (126), used some of Wertheimer's original gestalt figures as adapted by L. Bender for a "visual motor gestalt perception test" in the study of gestalt organisation during and immediately after insulin reaction and in the post-convulsive metrazol period. They found disturbances in gestalt function to consist of (1) a tendency to perseveration, (2) substitution of circles and loops for points, (3) substitution of curves for angles, (4) substitution of uninterrupted lines for dotted lines, (5) changes of angles into straight lines, (6) rotation of figure parts, and (7) spatial separation of gestalt units. Stainbrook and Löwenbach (140), using these same gestalt figures over the period of post-convulsive reintegration and getting repeated drawings of a gestalt model at various time intervals after convulsion, found similar disturbances. They also noted that the earliest attempts at copying the Wertheimer designs were reactions to the whole figures, a simple loop separated in space being made for each obvious element in the model. In the recovery sequence angularity was the next feature to be correctly represented, then the units of the figure were slowly brought into proper spatial relationships, and finally, near the end of the post-convulsive recovery period, the parts of the figure were represented joined together as in the model.

These same investigators also studied the return of the writing function following convulsion. Perseveration and more tendency to completion of the task for the patients' own signatures than for other suggested phrases were noticed in the early periods. The first post-convulsive writing attempted had in spite of tremor and incoordination all the formal characteristics of the individual's ordinary handwriting. Frequently patients reverted to writing the names associated with childhood or adolescence or to the diminutive forms of their names or to a phonetically simpler spelling. Married women frequently wrote their maiden names. Printed letters occasionally appeared, and once or twice a substantive within a sentence was capitalized.

Moore (95) has compared the reactions of a patient in a mild hypoglycemic state with the pretreatment behavior, using Street's gestalt

completion test, serial subtraction of seven from 100, and the Goldstein modification of the Koh's Block Design test. He found fewer figure interpretations of the gestalt test, the appearance of finger-counting as an aid in serial seven subtraction, and inability to "abstract" the designs of the Koh's blocks. It was concluded that analytic and synthetic thinking was impaired with greater dependence upon sensory impressions, a behavior consistent with Goldstein's hypothesis of the existence of a "concrete attitude" or loss of the "categorical attitude."

Fingent, Kogan, and Schilder (34) have asked schizophrenic patients to draw the figure of the Goodenough test during the awakening from the effects of metrazol seizures or from insulin coma. They have observed that the drawings show signs of organic confusion in gestalt perception and representation. However, the typically schizophrenic handling of the test is seen when the organic confusion expressed in the drawings of the early post-convulsive stage subsides. The conclusion is recorded, therefore, that the treatment introduces a new element, the organic confusion, into the picture and that the effect of insulin and metrazol is not a direct attack upon schizophrenic structure but upon "deeper seated structures" leading to their reconstruction and reorganization. If a concomitant reorganization of the schizophrenic process takes place, it is "reflected in the gradual loosening of the schizophrenic characteristics of the drawings."

Following Angyal's (6) neurologizing assumption that the symptoms of hypoglycemia may be encompassed within six insulin-shock syndromes, (1) frontal lobe, (2) ontogenetic, (3) aphasic-amnesic, (4) static-paresthetic, (5) coenesthetic, and (6) parieto-occipital, Gyfaras (55) has observed two kinds of spontaneous drawing disturbances in hypoglycemia. One group of disturbances is considered to be frontopolar in origin with evidence of elimination of inhibition, conventions, and schemes of thought, and a general reduction and regression to a more primitive personality is seen. This inferred regression is entirely absent from a second group, (the parieto-occipital syndrome), where disturbances of drawing due to the dysfunction of the "constructive faculty" prevail without infantile and primitive personality traits, probably a kind of drawing behavior reflecting a disorder like the constructive apraxia of Kleist.

Vujić and Ristić (146) have studied disturbances of colored after-images during hypoglycemia and they report that on the average the duration of after-images is reduced by 68% and that complementary color is lost in certain cases. In 50% of the subjects there was a temporary complete loss of after-images.

Kao and Lyman (76) found that electric shock treatment abolished in an eidetic patient the prolonged, mobile transformations of the eidetic imagery. Afterwards, the eidetic responses regained their former vivid and colorful character and returned to practically the same state as before treatment was instituted.

On the basis of electromyographic and cinematographic studies of single metrazol seizures, Strauss, Landis, and Hunt (141) divided the convulsion into a first clonic stage, a tonic stage, and a second clonic phase. The second clonic phase is considered to be like the tonic phase except that interruptions in the continuity of innervation appear. The first clonic stage is different from the second clonic stage and is probably due to cortical stimulation. The tonic stage corresponds to a state of decerebrate rigidity.

Kino (79) found that the characteristics of dermatographia did not change before and after single electroshocks, but Fortuyn (37) saw alterations of dermatographia in the hypoglycemic state, consisting of a decrease in the time of latency and the appearance of a broader red line than normally present. The white area surrounding the red line became regressive or was even absent.

Kino and Thorpe (80) also described the occurrence of the grasping reflex in post-convulsive stages of electrically induced seizures and found, quite interestingly, a difference in frequency of the appearance of this reflex in acute schizophrenia and in manic-depressive psychosis. It would be desirable to have further confirmation of this observation that the expression of a neurological reflex is selectively influenced in the post-convulsive state of patients with different mental diseases.

Using a modified form of the Rorschach test presentation, Stainbrook (137) was able to assemble composite Rorschach psychograms in many cases for each five minute interval following the onset of an electroshock convulsion. He concludes that progressive increase in productivity and accuracy of form conceptualization, disappearance of perseveration and simple color-naming responses which appear in the earliest records, and the gradual appearance of FC, or form-color responses following a phase of primarily CF, or color-form answers were the main Rorschach indices of change in the post-convulsive period. Movement responses were always the last Rorschach concepts to reappear in the records.

Flescher (36) studied the extent and duration of retrograde amnesia following electric shock in 18 early schizophrenics, giving various memory tests before the shock and then again at times varying from a few minutes before shock to four to seven hours after. It was concluded that

material which does not appear spontaneously or through association or recognition at the end of this time is permanently lost. Hemphill (60) investigated the characteristics of the recall of eight pictures presented to his patients about 30 minutes before the injection of metrazol and again at about an hour after the convulsion. He writes that no patient failed to recollect having seen the pictures and that no patient remembered less than three pictures and that three patients were able to describe all the pictures. He also insists that the absence of retrograde amnesia is easily demonstrated after electrically induced convulsions.

Ten patients treated by electric convulsive therapy were taught paired word associations by Zubin and Barrera (162) before treatment, and their retention was then tested after single shocks by recall, relearning and recognition methods. A control series of associates had been learned and tests of retention given a week before treatment. They found no significant saving in the experimental series, although the learning ability was not impaired. The recall and recognition scores also showed evidence of memory loss. They concluded that shock affects material learned immediately before the convulsion more than that learned less recently. Zubin (161) also introduced interference in the form of new associations to the original words and found that such interference was accentuated after shock. It therefore appeared that electroshock disorganizes but does not destroy memory traces. Rodnick (120), publishing the results of one of the few well-devised experiments of the effect of therapeutic shock upon habit systems in the human subject, persuaded 21 schizophrenes to learn two similar but antagonistic simple habits of moving the finger either to the right or left depending upon the frequency of a tone. Twenty-four hours intervened between the learning of the first habit and the similar but opposite second habit. Patients were retested one and a half hours after metrazol shock to determine habit dominance. He found a statistically higher number of reversals to the older habit in the shock group than in a similar control group. This indicated to him that metrazol exerted a differential effect with reference to older and to more recently acquired habits.

Wiedeking (153) used tests like Bourdon's attention test and a word-association test and demonstrated in normal subjects who voluntarily submitted to Sakel's technic of inducing hypoglycemia that a parallelism existed, with small doses of insulin, between mental impairment and falling blood sugar. With high insulin doses there was a continuous drop in mental efficiency regardless of the variation in the blood sugar level.

Subjective Description. Wiedeking (153) reported the subjective ex-

periences of three medical students who volunteered to subject themselves to insulin-induced hypoglycemia. They described feelings of marked hunger, heaviness of the limbs and general weakness, feelings of being tired and wanting to be left alone. The mood was one of apathy. Consciousness varied in phases with "clouding." A feeling of blankness developed out of difficulty in thinking. Disorders of sensation were frequent. Entoptic phenomena and pseudo-visual hallucinations occurred. On awakening, euphoria and a sense of having been saved from danger were marked.

Gillespie (47) published the results of an inquiry into the subjective sensations of 23 mental patients undergoing metrazol treatment, but because the patients' accounts tended to be distorted by their mental condition he, himself, underwent a metrazol convulsion. He described distinctly unpleasant feelings with headache, anorexia, and malaise for many hours afterwards. There also existed some retrograde amnesia even 10 hours after the convulsion.

Watkins, Stainbrook, and Löwenbach (147) reported a subconvulsive electric shock reaction in a 25-year old normal physician in which absence of somatic and mental complaints and the presence of amnesia and disorientation were the outstanding features. Immediate post-convulsive copying of a gestalt figure was accomplished initially by making dextrad circling movements, following which the subject attempted to "close in" on the model and to draw directly on the figure, behavior similar to that seen in psychotic patients in the early post-convulsive period. Some dysmnnesia in the ability to designate concepts was evidenced by the Rorschach test. The existence of anterograde amnesia was also displayed on the Rorschach blots by the continual repeating of responses which he had already given and which he had forgotten had once been given.

Fraser and Sargant (38) have published written accounts by schizophrenics at the end of an insulin treatment course in which their subjective experiences during their illness are detailed. No attempt was made to analyze these personal records and a reading of the five typical letters published gives no knowledge of how these patients interpreted either a single insulin reaction or the total treatment.

OBSERVATIONS DURING AND AFTER TREATMENT

General Description. Polatin and Spotnitz (115), describing an ambulatory insulin shock technic with which they treated 44 schizophrenic patients with 82% showing clinical improvement, write that such improvement occurs in four progressive stages: (1) improvement in spon-

taneity, (2) greater interest in the environment, (3) dissolution of psychotic ideas, and (4) readjustment to the ordinary ways of living.

Harris (58) reports that the characteristic picture seen in schizophrenic subjects treated by cardiazol was one of euphoria and mild overactivity. The euphoria is described as causing paranoid patients to forget their grievances at least for a time. This writer also remarks that cases of catatonic stupor passed into catatonic excitement or into hebephrenia if they did not recover and that mute and inaccessible patients began to talk, that untidy patients became cleaner, and that the unoccupied began to work. Schilder (126) thought that, with metrazol, catatonic symptoms very often disappeared first while paranoid ideas frequently remained.

Friedman (40), employing metrazol, stressed the changes revealed in spontaneous verbal and written expression in 70 chronically ill patients, some of whom began to write letters after having not done so for years. This observer also felt that there soon occurred a fear of somatic organ changes in almost every case, and he saw behavior which he described as "an expression and release of previously dissociated, immature, and pathologic sexuality." Alterations in affect, increased sensitivity, and sudden impulsive-destructive reactions directed either outwardly or towards the self and occurring in previously apathetic or stuporous individuals were also noted.

Feldman, Fiero, and Hunt (33), describing suspicion, agitation, fear, and fairly well-systematized ideas of persecution in patients before treatment, saw that, following treatment, these patients displayed personality changes from the schizoid characteristics to overt, well-balanced social attitudes. However, these changes were mostly only temporary. One case, interestingly, became psychotic again with a hypomanic extraverted reaction in contrast to the pre-treatment introverted schizoid personality. Blair (13) has noted that the firmly rooted delusions of the typical paranoid schizophrenic are rarely affected by shock treatment but that delusions of a superficial and bizarre nature attached to or resulting from changes of mood may disappear. Bain (8) also had little success with metrazol therapy when there existed either fixed delusions or pronounced apathy.

Cheney, Hamilton, and Heaver (22), in treating patients with metrazol, observed that the symptoms of depressed and agitated states were supplanted by a pathological though not marked elevation of mood. Slight hypomanic states usually followed recovery from depression and manics occasionally appeared slightly depressed after electric shock therapy in the hands of Kalinowsky, Bigelow, and Brikates (72).

There is general agreement that shock therapy is most valuable in the treatment of depressive states. As Kalinowsky (70) observes, a depression can be cut short at almost any time by adequate shock treatment.

Kant, Phillips, and Stolzheise (74), presenting a description of shock treatment in schizophrenia, state that hallucinations, body sensations, and all unusual symbolic expressions are the first symptoms to disappear during treatment, but they have observed that, although the voices cease, the ideas of persecution remain, and although the patient may be no longer tormented by "electric rays," the idea of being influenced is vaguely present. Silbermann (130) has also noted that hallucinations may often disappear during treatment, and Moore (94), who discussed the maintenance treatment of chronic psychotics by electrically induced convulsions at the rate of about four to six a month, described the temporary disappearance of hallucinations. Gruenberg (54) found in treating schizophrenics with high frequency current that towards the end of treatment auditory hallucinations became subjectively less loud, less clear, and more distant. He applied this treatment with some success in alcoholic hallucinosis.

By Rosas (122) used metrazol to treat hysteria and obsessional neurosis and concluded that the most resistant symptoms were those of a compulsive nature, particularly the obsessions and the phobias. Metrazol was used, too, by Owensby (103) in the therapy of homosexuality and lesbianism and he reported six such cases as "cured" with in some cases establishment of normal sex relations and no return of homosexual desires or tendencies for as long as 18 months after treatment. Liebman (84) subjected a psychotic, transvestic male homosexual to electric shock and obtained remission of the psychosis, disappearance of the transvestism, but only some inhibition of the homosexual behavior.

Weigert (149), who is a psychoanalyst and therefore more critically resistant to overenthusiasm about shock treatment, writes that the patients are changed in emotional behavior during shock therapy for a more or less limited time without having changed their fundamental attitudes towards life-problems. But Kalinowsky (70), too, agrees that, at least in psychoneurotic depressions, although the depression is favorably treated, the neurotic attitudes remain. Varying the emphasis somewhat, Schilder (126) thought that the "psychoses and the psychotic symptoms are not forgotten" but that the individuals have changed their emotional attitudes. Berrington (12) has published a few very interesting expressions from cardiazol-treated patients which substantiate the attitude change towards less intensity in the activation of

mental content. One patient, for example, said that "It (the treatment) has held my imagination in curb. There is nothing I am hunting for."

A probing psychodynamic approach in patients undergoing metrazol therapy does not seem to have the disturbing emotional aftermath that such an analysis frequently evokes in individuals not in convulsive treatment, according to Cheney, Hamilton, and Heaver (22). Eissler (31), who was primarily interested in an analytic evaluation of metrazol treatment, felt that schizophrenic patients so treated showed, in addition to lack of self-observation, a lack of emotional depth and a withdrawal from personal contacts. He also thought that these patients' dreams had, after treatment, degenerated to expressions of predominantly simple overt wish-fulfillment such as Freud originally believed characteristic of small children. And referring to changes in the dreams of patients undergoing shock therapy, an area of research which has received but little attention, Boss (18) believes that if there is no change in the dream-content of patients being treated by convulsive therapy the prognosis is poor.

Moore (94), treating some patients by a maintenance regime of four to six convulsions monthly for in some cases as long as two years, insists that clinical observation of these individuals shows no deterioration in personality or intelligence. This observation, however, is not objectively substantiated. Neymann, Urse, Madden, and Countryman (100) also saw no dementia or flattening of the personality among the patients of their recovered group of schizophrenics, manic-depressives, and chronic alcoholics subjected to electroshock therapy.

Kalinowsky (71) believes that all patients undergoing electric convulsive therapy show an early impairment of memory as well as emotional disturbances. He does not feel, however, that learning is grossly affected and cites an interesting case of a refugee physician who was treated for depression until he was deeply confused and had confirmed Petri's rule by losing his ability to speak English. Nevertheless, a few months later he passed a state board medical examination. Smith, Hastings, and Hughes (133) mention memory changes as always occurring to some degree with electroshock therapy, but they write that "these memory defects do not seem to be permanent." Schilder (126), too, thinks that the amnesia seen after metrazol treatment completely clears.

Whatever may be the reports concerning the complete disappearance of shock-induced memory impairment, no one who has talked to patients who have undergone electroshock treatment can doubt that there is a considerable amount of experience surrounding and during

the course of treatment which remains permanently inaccessible to memory. Quite parenthetically, it is interesting to note that Ingalls (68) successfully treated a case of hysterical amnesia by metrazol-induced convulsions. After the fourth convulsion, his patient is reported to have regained complete memory of his past.

Harris (58) remarks that the majority of his cases gained weight but that the weight gain was independent of mental changes. There occurred a marked weight increase even in patients who showed no recovery. Reports of gain in weight are perhaps the most consistent and universal treatment results described by users of shock therapy, of whatever type, and psychosurgery.

Early in the history of insulin therapy considerable attention was paid to the acute psychotic picture sometimes occurring during treatment. Sakel (125) called this the *activated psychosis*. However, as Kalinowsky (71) has observed, "all the known varieties of symptomatic psychoses occurring in infectious and toxic diseases can be seen in a long course of electric shock treatments." These states are reversible and, as Glueck and Ackerman (50) point out, these acute psychotic episodes are not like actual psychoses in that they are (1) acute and unstable, and because (2) the emotional display is genuine and reflects depth, (3) the mental content is chaotic, (4) the patient makes more direct demands upon the environment with less recourse to veiled symbolic expression, (5) the patient reacts to frustration of demands more positively and energetically and with more direct expression of hostility, and (6) the patient exhibits primitive infantile but strong transference.

Specific Description. Much of the early psychologic research in shock therapy centered about attempts to arrive at factors which might afford a basis for the selection of patients for successful therapy. Bolles, Rosen, and Landis (17), using the Vigotsky, Weigl, and BRL sorting tests on 19 schizophrenes treated by insulin, concluded that the patients whose performances were superior before treatment showed most improvement and that the patients with poor pre-treatment performance showed little or no improvement.

Skottowe (132) felt that patients who showed what he called *dys-symbole*, ("a state of mind which manifests itself by the inability of the patient to formulate his conceptual thoughts upon personal topics or to discriminate the gradation of his emotions in language which is intelligible to others . . . notwithstanding that he may be in a state of clear consciousness"), do not respond favorably to shock therapy. Thomas (143), believing *dys-symbole* to be pathognomic of true schizophrenia, observes that in 32 cases which he treated with either insulin or cardi-

azol, no case showing unequivocal signs of *dys-symbolie* made a good recovery, nor did any recovered case at any time show evidence of this kind of thinking.

Piotrowski (110), using the Rorschach test, felt that, on the whole, the difference in pre- and post-treatment records paralleled the clinical improvement. This improvement was manifested on the Rorschach test by (1) improvement in speed and ease of answering, (2) better logical content of responses and less mixing of description and interpretation, (3) an increase in the number and quality of movement responses, (4) an increase in the number and percentage of form-color concepts, (5) an increase in percentage of sharply perceived forms, and (6) good percepts and integrating capacity. Signs of predictive value for good prognosis were devised (112) and described as (1) variety, indicating no concepts used more than twice, (2) generic term, referring to patient's attention to logical hierarchy, (3) evidence, or self-critical evaluation of responses for "adequacy of fit," (4) color response, or at least one color interpretation except simple color-naming, (5) indirect color, or evidence of attention to color-areas, and (6) demurring, or holding back one or more responses. Elsewhere, Piotrowski (111) has reported that patients who give both color and human movement responses to the ink-blot tests have the greatest chance of benefiting from insulin therapy. As seen in the performance on the *concrete-abstract* tests, Piotrowski also concludes that on the pre-treatment records the patients who improved with shock therapy were functioning on "a higher intellectual and emotional level" than was the pre-treatment unimproved group.

Halpern (57) studied with the Rorschach inkblots 17 schizophrenes before and after insulin treatment and found that the improved and unimproved differed reliably in that the improved group gave greater number of responses, five times as many movement responses, more color responses, and a greater number of human concepts. Eisner and Orbison (31) also gave the Rorschach test before and after metrazol therapy and decided that the patients who benefited were "more emotionally inhibited, more constricted, and more socially withdrawn" than those who did not benefit. Moreover, these investigators felt that no matter what clinical improvement occurred these patients "remained unequivocally schizophrenic after metrazol."

Morris (96) administered the Rorschach test to 41 patients undergoing metrazol therapy and applied the chi square test for the validity of his differential signs and concluded that patients tending to remain unimproved gave 15% or more anatomy responses and more than two

color-form concepts and less than 70% good form responses in their pre-treatment records.

Kenyon, Rappaport, and Lozoff (77) used the Rorschach test, the Babcock deterioration index, and the Szondi test before and after metrazol treatment of three paretics. To the Rorschach blots the paretics reacted with less total color expression following treatment. The Babcock test scores improved with clinical improvement, but, of course, marked deterioration existed even in the fever- and metrazol-improved patients.

Kisker (81) indicated that the pre-treatment Rorschach picture of insulin- or metrazol-recovered patients was characterized by lack of concentration, numerous incidental remarks, good perception accompanied by uncontrolled, extensive associations, and an unevenness of performance. He also observed a falling off of the mean M or movement score after the start of pharmacotherapy and the appearance of confusion after 20-30 shock days. Like almost all users of the Rorschach blots in the study of shock-remitted psychotics, he noticed that some patients with good clinical improvement still showed psychotic characteristics in their Rorschach performance. Weil (150), too, writes that psychotic features appear in the Rorschach situation even after an insulin-treated patient appears clinically cured.

Using standard intelligence tests, Wittman (156) found that test scores increase progressively after metrazol treatment begins until the ninth convulsion when they gradually decrease. She and Russell also noted that the intellectual performance improved in interest, attention, and social responsiveness (157).

Wechsler, Halpern, and Jaros (148) made a comparative psychometric study of mental efficiency of schizophrenic patients immediately before and after insulin treatment. The performance on Wechsler's vocational interest blank, a test of counting by three's, naming words in three minutes, and a similarities and a directions test gave a correlation of .73 and a correspondence of 87% with a clinical appraisal of the patients' condition six to 18 months after the end of treatment. The increase in the number of "liked" occupations on the vocational interest list was particularly marked in the improved patients. Their analysis also suggested that certain patients may be harmed insofar as test performance after treatment is concerned. McNeel, Dowan, Myers, Proctor, and Goodwin (91) used a large battery of psychometric tests before, during, and after insulin therapy and concluded that a fair correlation existed between clinical psychiatric rating of post-treatment status and the psychometric rating. Luborsky (88), too, gave a battery

of 22 psychometric tests to 12 patients before, during, and after treatment by electroshock. He concluded that the profiles of the schizophrenic patients showed a general decrease in test scores from the line of zero change in the *before-during* comparison. This decrease was not as marked in the *before-after* contrast. The profiles of the depressive patients showed large score increases in the *before-during* comparison and further increase in the *before-after* contrast.

Sherman, Mergener, and Levitan (128) employed several memory tests consisting of designs, directions, and a reading paragraph and found a slight improvement in all tests after the conclusion of electroshock treatment.

The tests of Zucker and Herbert were used by del Pino (27) with schizophrenic patients during cardiazol treatment. He described improvement as being reflected in quicker appearance of images, easier representation of scenes, and in the absence of the deviation or replacement by other images such as occurred before treatment.

O'Connell and Penrose (101) studied the psychomotor efficiency in 30 patients treated with metrazol, quantifying the reaction time to an auditory stimulus, the tapping rate, and the strength of grip, and they concluded that in patients who showed before treatment marked incompetence in the tests, whether due to stupor or to agitation, improvement was greatest. They also found that the increase in the tapping speed was greatest in the early phase of treatment and that the amount of improvement decreased steadily as the number of convulsions increased. Subconvulsive doses of metrazol did not contribute appreciably to improvement in tapping scores.

Solomon, Darrow, and Blaurock (134) measured the blood pressure and electrodermal response during standardized interview situations before and after insulin and metrazol therapy and found, following pharmacologic treatment with clinical recovery, that in two groups of patients (one group showing pre-treatment psychological resistance to the examiner or to the test-situation and the other evidencing resistance involving perplexity in discussing or the evading of emotional problems, and all of whom gave large blood pressure reactions and small galvanic responses to several ideational stimuli in the interview), the autonomic responses were altered to show an unchanged blood pressure and larger palmar galvanic changes. Patients whose pre-treatment resistant attitudes persisted and who were unchanged by treatment revealed no change in the character of their autonomic responses. Patients who were overtly passive and cooperative both before and after treatment showed both blood pressure and galvanic

responses of large amplitude before treatment and these responses remained unchanged after shock therapy whether or not recovery took place. They concluded that improvement or recovery involves a decrease in inhibitory effects and favors increased sympathetic reactions in combination with and partially balanced by greater cholinergic response to ideational stimuli.

INTERPRETATIONS OF THE PSYCHOSIS-MODIFYING EFFECTS

Non-analytic Theory. The lack of enthusiasm in contemporary psychiatry for the various systems of psychologic description is amply confirmed by the occurrence of an almost complete adherence to psychoanalytic concepts in the reports of psychiatrists describing the effects of shock therapy upon psychotic behavior. Not all of these psychiatrists are psychoanalysts in the formal sense, and, hence, some of the writing is hesitant and tentative in the use of analytic constructs. However, the psychoanalysts, themselves, have been generally very keen observers of what happens to the shock-treated personality, but their descriptions are so admixed with analytic interpretations that in many cases the facts of behavior cannot be distinguished from opinions about the facts.

Nevertheless, various non-psychoanalytic interpretations of the process of symptom-remission following shock procedures have been put forth. Otto Pötzl, who made possible the easy introduction into psychiatry of Sakel's treatment, believed that the symptomatic disappearance of the psychosis was due to the disturbances of consciousness and to the associated impairment of memory. Myerson (99) also attributes recovery to the memory difficulty. He thinks that the "mechanism of improvement and recovery seems to be to knock out the brain and reduce the higher activities, to impair the memory, and thus the newer acquisition of mind, namely the pathological state, is forgotten. As the brain recovers, the well-established trends—those which are relatively normal—come back, but the incubus of more recent evolution and with less roots—so to speak—of thinking, feeling, and doing, remains away."

The neurologic descriptions of the interruption of "pathways" and of either the creation or abolition of disturbances in cortical neural "connections" and of the readjustment of the neuronal "shortcircuiting" supposedly associated with the abnormal behavior constitute some of the neurologic attempts to explain the remissions from psychosis observed following shock therapy.

Referring to the work of Löwenbach and the writer, Fulton (43)

considers electroshock treatment as a *functional ablation* of the frontal areas and links this kind of therapy with psychosurgery as, indeed, Freeman had done earlier. The psychological interpretation of the efficacy of electric shock treatment from this point of view is that this *functional ablation* of the frontal cortical areas removes some of the influence of these areas from active participation in behavior and thus psychologic projection into the future with the attendant anxiety is no longer easily possible for the subject.

Fear of the convulsive experience, particularly of the convulsions evoked by agents other than electricity, has been considered as an important motivation for the patient to control psychotic behavior in favor of more adequate reality-testing.

Psychoanalytic Theory. The psychoanalysts have generally stressed the punishment aspects of shock treatment, holding that convulsions may discharge energy derived from inwardly-directed destructive tendencies, particularly manifested in the depressed states, so that either the expression and relatively guiltless acceptance of hostility may then be possible for the patient or so that a relative increase in the strength of the life-instincts may occur. Weigert (149), for example, writes that the "cruelty of the super-ego is replaced by a sadistic attack on the part of reality" and so, by discharging the self-destructive urges from the inner conflicts, reality becomes again the object of libidinal cathexis.

Then, too, the primitive transference situation many times established following convulsions may give reassurance to the patient, as Wilson (155) indicates, that the good and beloved parent figures are real and that the hate and death wishes regressively generated around the primal negative super-ego images are not triumphant over the love-giving representations. Bychowski (20) also observes that the pathologic ego which has been distorted by the infantile desires of the id is partly destroyed or weakened by treatment. The resistance against reality is therefore decreased and the readiness for a positive transference is increased.

Abse (1, 2, 3) believes that the treatment situation evokes objective anxiety and that this threat of external danger orients the ego to the current reality-situation and results in an anxiety-motivated repression of the psychotic content so that, in schizophrenia at least, the patient is no longer "at the mercy of his complexes." The reestablishment of the functional supremacy of the ego-complex is thus achieved as a result of these repeated danger situations and the concomitant heightening of somatic sensation involved in the convulsive experience. In an

article by Schilder (126) the euphoria commonly seen following shock treatment has been ascribed to the overcoming of the death threat of the convulsion with the consequent feeling of a hypomanic joy of rebirth.

Almost all analysts are agreed that the aims of psychoanalytic therapy are in direct opposition to the apparent results achieved by shock therapy. The aim of psychoanalysis is seen either as the mitigation, and not the mere replacement in kind or even augmentation, of the cruelly primitive super-ego so that the patient may be able to bear the necessary burdens of reality, or psychoanalysis is directed to the weakening of repression and not to the reinforcement of the repressive mechanism of defense which the analysts see as the result of convulsion therapy.

ANIMAL EXPERIMENTATION

The experimental quest for an adequate theory which would make rational the empiricism of convulsive therapy as used in psychiatric treatment is probably best oriented around human subjects. Certainly there is now no lack of a human experimental population with which to work. Nevertheless, psychologists and other workers interested in the rationale of convulsive treatment will want to study by animal as well as by human experimentation the complex of psychologic happening which is included in the effects of artificially induced convulsions. The results of many such controlled investigations are in existence concerning the behavior of animals following convulsions induced by various procedures. Most of these studies refer primarily to the experimental effects upon learning and upon the determinants and cognitive structuring of habits.

In a study of the influence of metrazol convulsions upon maze-learning, Bunch and Mueller (19) found no significant differential effect upon the learning by rats of a 14 unit multiple T-maze. Heron and Carlson (63) substantially confirmed this observation, although Loken (85) indicated that both maze time and errors may be increased following these pharmacologically induced seizures. Stainbrook (136) found that the relearning maze time was greatly lengthened following a long series of electroshock convulsions although the relearning errors were no greater than in a control group. The differentiating ability of the dog in a conditioning situation is reported to be disturbed immediately following the administration of metrazol, but, according to Rosen and Gantt (123), discriminatory efficiency tends to return with recovery from the effects of the convulsion. Kessler and Gellhorn (78)

have shown that convulsions reactivate in the rat a conditioned response previously inhibited by non-reinforcement. Rose, Tainton-Pottberg, and Anderson (121) administered a series of insulin shocks to a well-trained sheep in which a conditioned reflex had been standardized in tests extending over a period of seven years. They found that, following the hypoglycemic treatment, the conditioned reflex which had been almost entirely absent for one year reappeared with abnormal vigor. Interval leg movements, associated ordinarily with experimental neurotic behavior, appeared for the first time in the history of the animal and became a relatively permanent part of the animal's reaction to the experimental situation. Page (104) and Stainbrook (135) have also observed signs of abnormal behavior in animals following electroshock convulsions, and Stainbrook and de Jong (138) saw reactions of the rat indistinguishable from the state of "experimental catatonia" which ensued immediately after the cessation of an electroshock convulsion.

Riess and Berman (117), using a relatively complex and a simpler maze, concluded that insulin shock has a greater disintegrating effect on a less well-fixed habit than upon one of greater fixation, that the disintegrative effect is greater on the learning retention of a longer and more difficult maze than on a shorter and easier maze, and that hypoglycemic treatment has a disintegrating effect upon the initial acquisition of a maze habit. Siegel (129), however, concludes that "the retention of a 'barely learned' simple response in the rat is not affected by a series of electroshock convulsions," and he similarly reports that the ability to learn a simple habit is also not affected by such convulsions. Stainbrook and Löwenbach (139) have indicated that in a simple water-maze a long series of either electroshock or noise-fright* convulsions does not alter the maze behavior of the rat insofar as error scores are concerned; time scores, however, are significantly changed.

All experimenters with convulsive technics who use animals in controlled learning situations must take into account the change in the behavior of the animal after it has been "shocked" a few times. The increase in maze time following such procedures is very probably a reflection of this behavior change. The conclusions of Riess and Berman, particularly, being one of the few reports showing a permanent shock-destructive effect upon already learned maze habits, must be weighed by this consideration, especially since they saw that insulin had an unfavorable effect upon the initial acquisition of the maze habit

* Noise-fright is used to describe the reactions in the rat which are called *audiogenic*, *audioepileptic*, or *abnormal behavior* by various other investigators.

as well as upon learning retention. The change in maze behavior reported in the other situations measuring learning retention may have been due primarily to this general behavior change and not to a direct "shock" effect upon the habit system. Certainly, in a water-maze with constant escape motivation, it is impossible to significantly alter a well-trained maze habit, insofar as error scores are concerned, beyond an immediate post-convulsive period of shock effect varying with the spacing and number of convulsions given.

Obviously, in all these studies on the behavior of animals after convulsive treatment, both time and error scores should be used in quantifying the results and a description of the total behavior of the animals with a few simple operational specifications of such behavior should be included.

The field of animal experimentation either with reference to the effects of shock upon habits in the elaboration of a theory of shock therapy or in relation to the use of convulsions in experiments designed to test various postulates of the theories of learning is currently quite open, and experiments of this nature should be extraordinarily fruitful for the pursuit of both objectives.

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THE USE OF THE WECHSLER-BELLEVUE SCALES: A SUPPLEMENT

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In the July, 1945 issue of the *Psychological Bulletin*, Rabin (17) described in succinct fashion certain of the findings that have come about with the use of the Wechsler-Bellevue Scales with normal and abnormal persons. He stated that the review was "... an attempt to coordinate and summarize the findings to date ..." (17, p. 410). On examination of the literature through 1944 it appears that fifteen research studies and clinical applications have not been discussed. One of these studies appeared in 1941, two in 1942, five in 1943, and seven in 1944. The purpose of this note is to supplement and to amplify the material given in the previous article.* The material is organized following Rabin's plan of presentation with the addition of two sections, one on use in the Armed Services, and the other on the rationale of the various subtests. Assuming accuracy of report, specific studies previously presented will not again be examined with the exception of certain studies of Lewinski about which confusion might otherwise exist.

COMPARISON WITH OTHER TESTS

Five additional studies which present comparisons between the Wechsler-Bellevue Scales and other measures appeared prior to 1945. The relationship between the Herring Revision of the Binet-Simon Tests and the Verbal Scale was examined in two studies by Lewinski (10, 14), who used as subjects 100 recruits referred to a Naval clinic because of suspected mental retardation. Scale A and Scale B of the Herring Revision showed correlations of .65 and .64 respectively with the Verbal Scale notwithstanding a limited intellectual range. For the two Herring scales the difference in IQ points from the Verbal Scale was less than 5, in 55 and 40 per cent respectively and more than 15, in 5 and 12 per cent. The four mean IQ's were in the neighborhood of seventy, with the Wechsler IQ's higher in 58 and 76 per cent of the cases. Goldfarb (5) administered the Revised Stanford-Binet, Form L, to 60 superior foster home children between the ages of 11 and 17 and found the IQ correlation to be .86, .80, and .67 with the full, Verbal, and Performance Scales respectively. In 70 per cent of the cases the Stanford IQ was higher than the Wechsler-Bellevue IQ with

* The bibliography is limited to studies appearing in 1944 or earlier years.

the mean of the former being almost 5 points higher than that of the IQ on the full Wechsler-Bellevue Scale. Goldfarb concluded that the low scores on the Wechsler-Bellevue make the test relatively ineffective in discriminating among a group of superior adolescents. A further study comparing Stanford-Binet and Wechsler-Bellevue scores of negro and white criminals by Maizlish (16) has as yet appeared only in abstract form. Incidental to a comparison of narcotic addicts and matched hospital attendants, Brown and Partington (2) calculated for the entire group of eighty-four the correlations between the Wechsler-Bellevue and other tests. Among these were representative correlations of .66 with the USPHS Number Series Completion Test, .63 with the USPHS Paper Form Boards, .60 with the USPHS Maze Test, .59 with the Ferguson Formboards, .48 with the Knox Cubes, and .41 with Healy Picture Completion Test II.

Rabin, in his subsection on clinical status (17, p. 412), confuses two studies by Lewinski. The Lewinski study cited (12) is concerned with a comparison of the Kent Oral Emergency Test and the Verbal Scale. This study is summarized in Rabin's subsection on correlation with other tests (17, p. 411). Contrary to Rabin's statement, this article contains no mention of performance on individual subtests. The reference not cited is a study of the variability of the subtests of the Verbal Scale (11) to be discussed later. Rabin's quotation in regard to Digit Span (17, p. 412) is found in this latter study. Still another study by Lewinski (13) omitted by Rabin is relevant to the problem of clinical status. The Wechsler Verbal Scale was given to 451 Naval recruits referred to a neuropsychiatric clinic for an examination to determine their fitness for duty. The recruits' scores were grouped on the basis of Verbal Scale IQ into categories of Normal, Dull Normal, Borderline, and Mentally Deficient. Critical ratios between the mean scores of these groups for each individual subtest were calculated, and all found to show statistically significant differences between immediately adjacent groups. It will be noted that this study went beyond that of Wechsler, Israel and Balinsky (22) in that it investigated the discriminatory value of the verbal subtests at the four levels of intelligence mentioned.

In resume, it would appear that fairly high correlations are found between the Wechsler-Bellevue Scales and verbal measures of intelligence but that the correlations with performance type scales are somewhat lower, although still substantial. The trend reported by Rabin of relatively higher Wechsler-Bellevue IQ's for duller subjects and relatively lower ones for brighter subjects is substantiated. Adequate dis-

crimination of each of the verbal subtests at different intellectual levels is obtained.

TEST RESULTS AND CHARACTERISTIC PATTERNS OF SPECIAL GROUPS

The studies reported in this section are concerned with the varying scores obtained on the subtests of the Wechsler-Bellevue Scale in various personality and behavior disorders. As Rapaport *et al.* point out, "one cannot expect every case within the same nosological group to have identical test-patterns; but one can expect that in any nosological group, a large share of cases will have, as a reflection of the disorder, similar impairments of certain intellectual functions, and these impairments will become evident in the intelligence test scores" (19, p. 23).

The intertest variability of the scores of 158 mentally deficient and 189 borderline defective Naval recruits on the Verbal Scale was reported by Lewinski (11). In the mentally defective group the mean score on the Arithmetic subtest was significantly lower and Comprehension and Similarities significantly higher than the other subtests. The comparatively low score on Arithmetic corroborates the findings of Magaret and Wright (15) and Wechsler *et al.* (22). Cleveland and Dysinger (3) studied the Wechsler-Bellevue scores of 20 institutionalized senile patients with a mean age of 75.1 years. Many of the subjects made low or zero scores on the Performance subtests, which confirms the finding reported by Rabin (17) of a more consistent trend to decline in Performance subtest scores with age. Their results on the individual subtests disagree with those of Wechsler (21) since they found results contrary to his claim that Similarities did not "hold up" with advanced age, and that Object Assembly did do so. Unfortunately, only an abstract is as yet available of the study of Van Vorst (20) who used as subjects a relatively small number of delinquent boys who had been diagnosed as psychopathic personalities. He states that the results "... do not appear to support the claims ... to an extent which would justify defining any characteristic response pattern for the psychopathic personality."

The most extensive and detailed study of scatter of Wechsler-Bellevue scores yet to appear is that of Rapaport and his collaborators (19). Their nosological groups included paranoid and unclassified schizophrenics, both further subdivided into acute, chronic and deteriorated cases; simple schizophrenics; paranoid conditions; preschizophrenics divided into coarctated and over-ideational; depressives further subdivided into psychotic, involutional, severe neurotics and

neurotics; hysterics; anxious and depressed neurotics; mixed neurotics; obsessive compulsives and neurasthenics. In addition they used a control group further subdivided into smaller groups on the basis of trends in the direction of maladjustment exhibited. Although including a study of mean scatter and of the presence of extremely high or low weighted scores, principal attention was focussed upon the deviation of the scores from the level of the vocabulary score. Extremely detailed findings on each of the nosological groups prevent any adequate short description. A quotation from one of their summarizations will illustrate both their findings and their rationale of these findings.

The deteriorated unclassified schizophrenics show extreme impairment on almost all of the subtests. Comprehension and Digit Span are greatly impaired; Arithmetic is even worse; and worst of all are Picture Arrangement, Picture Completion, and Digit Symbol. In striking contrast, they show relative efficiency on Information, Object Assembly, and, to a lesser extent, Similarities and Block Design. In general, the scatter of the Deteriorated group may serve as an exaggerated representation of the schizophrenic pattern: impairment of judgment (Comprehension), concentration (Arithmetic, Picture Completion, Digit Symbol), and planning ability and anticipation (Picture Arrangement).

THE RATIONALE OF THE SUBTESTS

Very little attention has been paid to the psychological functions tapped by the subtests except in the formulations of Wechsler (21) and Rapaport *et al.* (19). The latter disagree with Wechsler on the bipartite classification into verbal and performance subtests. The basis for their difference of opinion is clinical experience, theoretical considerations and statistical findings, although the latter is limited to inspection of mean scores and standard deviations on each of the subtests for 261 clinical and control cases. They conclude that both the Verbal and Performance Scales should be subdivided into two parts, all four "groups" differing one from the other in mean score and standard deviation. Vocabulary, Information, Similarities, and Comprehension are designated as the *Essentially Verbal* group since their common feature is the requirement of verbal presentation and response. The remaining two subtests of this scale, Digit Span and Arithmetic, are referred to as the *Attention and Concentration* group because of their presumed relationship to these functions. Verbalization is considered merely to be the form of communication. The *Visual-motor Coordination* group is so characterized because performance on the subtests, Object Assembly, Block Design, and Digit Symbol is said to depend upon coordinated visual-motor activity. The *Visual Organization*

group, Picture Arrangement and Picture Completion, requires no essential motor activity, but instead demands this function. Each subtest is also considered as calling into play certain functions or set of functions. For example, Digit Span is considered primarily a measure of attention, Similarities a measure of concept formation, and Arithmetic a measure of concentration. In many of their formulations they differ sharply with Wechsler (21) who considers, for example, that Digit Span is a measure of memory as well as attention, and that Arithmetic is a measure of reasoning.

USE IN THE ARMED SERVICES

The Wechsler-Bellevue Scale has been used extensively in the Armed Services (4, 7, 8, 10, 11, 12, 13, 14). In fact, as Hunt *et al.* say in reference to the detection of feeble-mindedness it "... has been adopted as the standard test for such use in the Navy" (8, p. 478). Both Hunt *et al.* (8), and Hildreth *et al.* (7) describing the psychometric procedures used at Naval Training Centers, give considerable prominence to the Wechsler-Bellevue Scale. As early as 1941, Rapaport (18) suggested a possible use of items from the Wechsler-Bellevue in examining selective service registrants suspected of feeble-mindedness. In three case histories with implications in regard to military selection, Knight *et al.* (9) described the use of Wechsler-Bellevue findings along with those from a variety of other measures. The Army has in recent years used a revision and extension of the Wechsler-Bellevue Scales, the Wechsler Mental Ability Scale (23). As described in Greenwood *et al.* (6), it consists of sixteen subtests, seven verbal and nine non-verbal or performance tests. It is so standardized that a selection of a lesser number of subtests appropriate to the situation may be made. Presumably in the immediate future we may expect to see many research studies appearing which use this presently restricted measure.

SUMMARY AND SUGGESTIONS

That the results of studies reported here would in some measure coincide with the results of those previously summarized is hardly surprising. A summarization showing considerable agreement with that of Rabin is natural, not to say inevitable. Repetition is obviously unnecessary. Therefore, a rounded summary is not attempted. Instead certain summary statements or cautions unreported previously will be stressed, based upon studies in both reviews. There are, however, certain points of disagreement of emphasis which should be mentioned.

As part of the first sentence of his summary Rabin states, "... the

Wechsler-Bellevue Scales . . . have supplanted some time-honored diagnostic tools" (17, p. 419). This rather broad and undocumented statement is open to question. It would perhaps be more correct to say that in the testing of adults they have *supplemented* other diagnostic devices such as the Stanford and Revised Stanford-Binet, the Babcock Test, and the Arthur Performance Scale, not to mention the whole array of less directly comparable measures which modern clinical procedure affords. In this connection it might appear to many that the dismissal of age-level intelligence tests as "hotch-potch scales" (17, p. 413) is too cavalier a treatment of such measures as the Stanford and the Revised Stanford-Binet Tests. He goes on to say in regard to the Wechsler-Bellevue " . . . that it tends to differentiate better than other measures between the dull and feebleminded" (17, p. 419). It may be this will prove to be the case; as yet the evidence on the matter is hardly conclusive. For example, the one study described by him on the relative merits of Stanford-Binet and the Wechsler-Bellevue Scale in clinical effectiveness as an aid in the diagnosis of mental deficiency was performed at the Bellevue Hospital (1). Although tempered by the social and psychiatric data, there was a possible predisposition on the part of the psychologists to urge, and the psychiatrists to accept, as more valid the findings of a scale developed at their own hospital. For a second group in the same study this factor could not operate since the Wechsler-Bellevue was given to the subjects two or three years after the recommendation for commitment or non-commitment was made. For the 36 cases on which both measures were available the biserial correlation coefficient for the Wechsler-Bellevue was $.720 \pm .086$ while that for the Stanford-Binet was $.611 \pm .103$. Presumably the difference between these correlations is statistically non-significant.

The differences of opinion in regard to rationale between Wechsler and Rapaport are both encouraging and disconcerting. Encouraging, because they show that attention is being given to the meaning of the subtest scores obtained; and disconcerting, because they show that much remains to be done before a thoroughgoing rationale for the various subtest scores is established. Some might find the emphasis on a pattern of "function" a retrogressive trend in clinical psychology smacking too much of a faculty concept of mental organization. Further experience, clinical, experimental and statistical, will eventually allow more final judgments to be drawn. Studies showing whether scores on the subtests are correlated with successful performance in other situations which require the same functions seem indicated. Factor studies are also applicable. There is no doubt, even today, that individual

clinical use seems to become more meaningful upon application of some conception of rationale. Enough evidence exists to show that a pattern of variations in scatter on the Wechsler-Bellevue Scale, in part, is produced by a pattern of psychopathy, but it must be remembered that even if the subtests were pure "functional entities," differing educational backgrounds, and cultural environments, unrelated to psychopathy would tend to blur the findings making the patterns indicative rather than invariably diagnostic. The Wechsler-Bellevue Scale is a valuable clinical instrument but it does not allow the substitution of a list of diagnostic signs for clinical acumen.

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PSYCHOLOGY FOR THE ARMED SERVICES

A SPECIAL REVIEW*

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This is a third book published under the sponsorship of the Emergency Committee of the National Research Council, following as it does the highly-successful *Psychology for the Fighting Man* and *Psychology for the Returning Serviceman*. The original manuscript was drafted by the summer of 1944 under the general editorship of Boring, and was revised in the course of the fall and winter by other psychologists and by military experts, including Colonel Joseph I. Greene and Colonel Edward L. Munson, as well as by the Science Service expert, Miss Marjorie Van de Water.

The present book is somewhat more factual and less advice-giving than the *Psychology for the Fighting Man* and considerably so than the *Psychology for the Returning Serviceman*. It "is intended as a textbook written on the college level, but also as a book in which the military and naval applications of psychological principles and the basic principles themselves are more fully developed than in the earlier book. It was believed that a single book might be equally useful as a textbook and handbook of psychology for general use by members of the Armed Services, not simply for instruction but also for individual reading and reference" (p. ix). In organization this book follows that of *Psychology for the Fighting Man*, almost chapter by chapter. The first ten chapters, largely on sensory functions of military value, present practically the same contents, with here and there some more details and often a rephrasing of passages to decrease their newspaper and increase their textbook character, without sacrifice in clarity or readability. The same is true for some of the remaining of the twenty-four chapters. Of those chapters receiving much fuller or even reorganized treatment, one should mention especially those on *Selection of Men* (chiefly by Bingham and Harrell), on *Learning* (Seidenfeld), on *Personal Adjustment* (Hunt, Abrams, Doll, and Seidenfeld), on *Assessing Opinion and Discovering Facts* (Edwards); and there is an added chapter on *Differences Among the Peoples of the World* (Klineberg, Child, and Canady).

Certain questions naturally arise. The first that psychologists ask is: Is the material sound, scientifically and professionally? A few hasty assertions may be found in it. Examples: "It is doubtful that the

* COMMITTEE OF THE NATIONAL RESEARCH COUNCIL. *Psychology for the armed services*. Washington: The Infantry Journal, 1945. Pp. 519.

muscles outside the eyeballs, the muscles that move the eyes, very often cause fatigue" (p. 33), "An after-dinner cup of coffee never kept any man awake because of the caffeine in it" (p. 222), and, "There is no evidence that the sexual act in animals is learned" (p. 398); but such assertions are almost inevitable in a book pitched at the level of this one—and these in particular are perhaps not to be disproven as yet by references to experimental data. Then there are certain misleading manners of statement that are likely to falsify the reader's understanding of psychological phenomena. Examples: "The brain of the man who owns the retina sorts the colors out into objects" (p. 74), "Attention picks out one set of impressions and puts them together into one object" (p. 75), or, "It was discovered that they [rats] have mental maps" (p. 157). But over against such concessions to the non-technical reader there are warnings against too hasty conclusions, as in the sentence, "The experimental results on this matter [of heavy smokers and their supposed need for more rapid breathing to get their needed amount of oxygen] are not, however, conclusive" (p. 219). And it can certainly be said that for factual soundness, *Psychology for the Armed Services* does not disappoint the expectations originally engendered by inspection of the reassuring list of collaborators.

The book before us is explicitly intended to serve as a textbook. Is it, then, teachable? That the intent is realizable is obvious enough. Each chapter and topic is organized to high degrees, with frequent employment of centerheads and sideheads that suggest didactic "points" for the student to set down and for the instructor to use as a skeleton for his presentations. Transitions from chapter to chapter are smooth enough. Not least important are the fairly generous illustrations and even a few simple tables, of wide range, whether taken from technical psychological journals or from supplies of military photographs—all of these being admirably apposite, and introduced to inform more than to entertain. The references appended to each chapter are well-selected and should be of real help to teacher and student.

Of recent years scientists have become more aware than formerly of the great gap between technical knowledge for the scientist and popular knowledge for the layman, and the urgency of building bridges between the two. To select those developments in scientific knowledge that are most relevant to human problems, to reorganize their statement into forms both understandable and interesting to the layman, and to make these cheaply available, is no mean achievement. And in the present book, together with *Psychology for the Fighting Man*, psychologists have before them effective examples of how the job can be done, and at two levels.

As in the *Psychology for the Fighting Man*, over a fourth of this book is devoted to the study of sensory and sense-perceptual processes. It seems not unlikely that that is fair reflection of the important role that seeing, hearing, smelling, and equilibration play in the combat activities of servicemen. But that it should be taken as pointing an error in the allotting of less space to those functions in textbooks of more general psychology, need not follow, as some may suppose (2). Indeed, another recent book on military psychology (3) allots only about 10 per cent to them.

Psychology for the Armed Services is easily distinguished from two other recent books by American psychologists for use in military instruction. Its scope is much more inclusive than is that of the Pennington, Hough, and Case book (4); and by the same token it is addressed to a wider audience, including both commissioned and enlisted men. On the other hand, its avenue of approach is distinctly different from that of Meier's (3) in that psychological rubrics determine its division into chapters, whereas discussions in the latter text take off from military problems and phases. The present work could be used in combination with either or both of the others mentioned.

The cooperative sponsoring of this book and its predecessors, both in the writing and in the distribution, by military and by scientific authorities, is a hopeful augury for applied psychology.

As a bookmaking job—paper stock, type, format, and binding—*Psychology for the Armed Services* is excellent.

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THE EFFECTS OF SCHOOLING UPON IQ

A NOTE ON LORGE'S ARTICLE

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In a recent publication,¹ Dr. Irving Lorge has presented data which have been widely hailed as demonstrating the thesis that "education increases the IQ." Such an interpretation of his study is entirely fallacious, and it would be most unfortunate if it were generally accepted. Lorge has, indeed, shown what psychologists have long known, namely, that group intelligence test *scores* increase with the amount of education. Moreover, he has been able to demonstrate from his data that when boys are roughly matched for mental ability at age 14, subsequent education is effective in increasing the group intelligence test scores of the better trained.

I have no quarrel with this last conclusion, although I think the effect of education is somewhat less than Lorge represents it to be. I do strongly object, however, to his use—or misuse—of the term "IQ." The uncritical impression gained from Lorge's paper, namely, that "education raises the IQ," has been aired in the newspapers, and is already being parroted by those who would like to believe that the school book is more powerful than original nature. Unfortunately, the correction of a wrong notion often requires the presentation of technical detail which is, in general, carefully avoided by the loquacious but untrained reader. As a result, a popular fallacy, once established, is extremely hard to eradicate. The present instance offers a good illustration of this fact, and is another reason why the scientific writer should be extremely careful what he puts into print.

There are two conclusions to be drawn from Lorge's report—the one true, the other false. I shall state these conclusions in order and summarize briefly the evidence for each.

Conclusion I; The more recent and the more extensive a person's education, the better he is likely to perform on tests requiring reading, information, vocabulary, arithmetic—i.e., on tests involving words and numbers.

This general result has been known for a long time; and any other finding would certainly seem quite unlikely. I shall cite evidence from only one source:—the extensive data furnished by the Army Alpha test, a verbal examination administered to nearly two million men in 1917–18. The correlation² between extent of schooling and Alpha

¹ LORGE, IRVING, Schooling Makes a Difference, *Teachers College Record*, 46: 1945, 483–92.

² YERKES, R. M., Ed., *Memoirs National Acad. of Sciences*, 1921, 15, 748.

score in a group of 48,102 recruits was .74. Among white enlisted men, scores in Alpha increased steadily with education: for men who had completed 5–8 grades, the mean was 51.1; for men who had completed high school, 92.1; for men who had completed college, 117.8. Scores made by officers declined steadily with age (recency of schooling); clerks (workers with words and numbers) scored higher than machinists (workers with tools); chaplains higher than dentists.

The evidence for the influence of education upon verbal intelligence tests seems, then, to be clear and convincing. Lest the educator become too complacent, however, I mention in passing that officers who reported schooling of 4 grades or *less* scored 20 points higher on Alpha (112.5) than white enlisted men who had completed high school (92.1). Perhaps there is food for thought in the suggestion that even a high school education is not always a substitute for brains in the first instance.

Lorge's data warrant special consideration since he has studied the effects of schooling upon test scores of men who 20 years earlier were equated (at least roughly) in group intelligence test score. It will be helpful to review briefly Lorge's experimental material in order to provide a proper setting for subsequent discussion. In 1921, 863 boys around 14 years of age were given a series of abstract intelligence, mechanical aptitude and clerical ability tests. In 1941—20 years later—131 of the original 863 were located and induced to take the *Otis Self-Administering Test, Higher Examination, Form B* (time limit: 20 minutes), and *Part III* of the *Thorndike Intelligence Examination for High School Graduates, Form V* (time limit: one hour). According to Lorge, this sample of 131 was not significantly different in mean achievement or in variability from the original 863, and hence was fairly representative of the larger group. The educational achievement (highest grade completed) of these 131 survivors varied from 8 grades to 17 or more (beyond college), and the opportunity is afforded, therefore, to discover how differences in extent of schooling affected the 1941 scores of boys equated for test score in 1921. The 131 "boys"—34 years old in 1941—were classified into six groups on the basis of their 1921 group intelligence test scores; and 1941 scores on Otis SA were tabulated for various levels of academic achievement.

Table I reproduces Lorge's Table I, and Table II is my summary of certain of these data. Consider the scores in Table I, column 89–98, the data especially cited by Lorge. A total of 23 boys out of 131 scored from 89 to 98 on the 1921 test. Of these 23 the 2 who completed grade 8 had a mean score of 39.0 on the 1941 Otis test; the 2 who completed

TABLE I*

AVERAGE SCORES ON THE OTIS SELF-ADMINISTERING TEST OF MENTAL ABILITY, HIGHER EXAMINATION, FORM B (20-MINUTE TIME LIMIT), TAKEN IN MAY 1941, CLASSIFIED BY SCORE ON THE 1921-22 TEST OF ABSTRACT INTELLIGENCE AND BY HIGHEST GRADE COMPLETED BY MAY 1941: FOR 131 BOYS WHO MAY BE CONSIDERED A REPRESENTATIVE SAMPLE OF THE VOCATIONAL GUIDANCE GRADE GROUP

		Score on 1921-22 Test of Abstract Intelligence									
Highest	49-58	59-68		69-78		79-88		89-98		99-114	
Grade Com-	<i>Otis</i>	<i>Otis</i>		<i>Otis</i>		<i>Otis</i>		<i>Otis</i>		<i>Otis</i>	
pleted by 1941	<i>Score</i> <i>N</i>	<i>Score</i> <i>N</i>		<i>Score</i> <i>N</i>		<i>Score</i> <i>N</i>		<i>Score</i> <i>N</i>		<i>Score</i> <i>N</i>	
8	14.0 4	22.0 4		20.7 9		26.4 5		39.0 2		33.0 1	
9	19.0 1	19.5 2		14.5 2		31.1 8		38.0 2		29.0 1	
10	24.0 1	22.0 4		25.1 9		28.5 8		37.0 4		46.5 2	
11 or 12	21.0 1	26.0 1		31.7 3		31.0 9		41.0 3		34.0 1	
13 or 14		22.0 1		26.0 3		34.7 4		41.7 4		37.5 2	
15 or 16		34.0 1		27.0 1		39.5 6		53.5 2		50.8 5	
17 or more				38.0 3		46.0 5		54.5 6		43.0 1	

TABLE II

SUMMARY OF CERTAIN DATA FROM TABLE I

Highest Grade Completed by 1941	Intelligence Test Scores: 1921-22									
	56-68		69-78		79-88		89-98		99-114	
	<i>Otis</i> <i>Score</i>	<i>N</i>	<i>Otis</i> <i>Score</i>	<i>N</i>	<i>Otis</i> <i>Score</i>	<i>N</i>	<i>Otis</i> <i>Score</i>	<i>N</i>	<i>Otis</i> <i>Score</i>	<i>N</i>
8- 9-10	21.5	10	22.1	20	29.0	21	37.8	8	38.8	4
13-16	28.0	2								
15-17 plus			35.3	4	42.5	11	54.3	8	49.5	6
<i>Difference:</i>	6.5	12	13.2	24	13.5	32	16.5	16	10.7	10

* From Lorge, *op. cit.*, p. 487.

grade 9 had a mean score of 38.0 on the 1941 test, and so on. The 6 boys who completed 17 or more grades (college and beyond) had a mean 1941 score of 54.5. Averaging the three entries at the top and the two entries at the bottom of this column we find (Table II, column 89-98) that the 8 boys who completed 8, 9 and 10 grades had a mean 1941 score of 37.8, while the 8 boys who completed 15 or more grades had an average 1941 score of 54.3. These two groups differed by 16.5 points on Otis SA in 1941, and this difference Lorge attributes to the better schooling of the second group. Several comments are in order:

1. The 16 boys in column 89-98, Table II, were not *strictly* equated in score in 1921. Their scores, in fact, show a 10 point range (89-98), while in 1941 the range between the 8 with the best and the 8 with the poorest education is 16.5 points. These ranges, to be sure, are not directly comparable; the tests are in different units and the second range is between two selected means and is restricted as compared with

the original range of 10 points. It seems reasonable to infer, however, that had the 16 boys been more exactly equated in 1921 score, the gain of 16 points might have been somewhat reduced. Evidence for this inference can be found in the other columns of Table II:—differences in Otis SA score attributable to 7–8 years of schooling are 6.5, 13.2, 13.5, and 10.7. Lorge, incidentally, gives comparative data only for the column in which the change is greatest.

2. It seems clear that 7–8 years of additional schooling *did* make a difference in the performance of these 8 subjects upon group intelligence tests. But the change wrought by education is certainly modest and hardly warrants a feeling of smugness on the part of their teachers.

3. Any conclusion as to the effects of schooling upon the Otis test scores of subjects equated for intelligence level 20 years earlier must necessarily be qualified by the small sizes of the samples. The 16 boys cited above bear a heavy burden; and results from even 131 subjects can at best be regarded as suggestive rather than conclusive.

Conclusion II: "Schooling raises the IQ." There is no evidence in Lorge's report to substantiate this statement.

We have seen that the 8 boys who scored between 89 and 98 upon an intelligence test in 1921, and whose subsequent schooling was 8–9–10 grades, had an average score of 37.8 on the Otis SA in 1941; and that the 8 boys who scored between 89 and 98 upon an intelligence test in 1921, and whose subsequent schooling was approximately 4 years of college, had an average score of 54.3 on the Otis SA in 1941. According to tables supplied for the Otis SA examination, a score of 38 is *equivalent* to a "Binet IQ" of 103, and a score of 54 to a "Binet IQ" of 115. The first score, namely, 38, is also found from the tables to be *equivalent* to a "mental age" of 16–5; the second score of 54 to a "mental age" of 18–5. Comparison of these two MA's and two IQ's furnishes the basis for Lorge's statement that for boys of "equal" intelligence at age 14, 7–8 years of schooling produce an increase by age 34 of "two full years" in MA (i.e., 16–5 to 18–5), or a change in IQ from 103 to 115.

It is worthwhile examining further this entirely erroneous conclusion. Psychologists conversant with test construction know that rarely if ever is an MA or IQ derived from a group test even remotely comparable to the IQ achieved on Stanford-Binet. This is recognized by Otis himself, who writes³ "The term *Mental Age* (capitalized), however has now come to have a special meaning and to denote measures of mental ability—i.e., scores—in the Binet-Simon tests. Binet mental

³ OTIS, A. S., *Manual of Directions, Otis Self-Administering Tests of Mental Ability*, Yonkers, World Book, 1928, p. 4.

ages below about 13 years are true mental ages. Above that, especially above 16 years, they are merely scores." (Italics mine) Again, Otis writes:⁴ "... IQ's as sometimes derived from group tests of mental ability bear little relation to IQ's derived from the Binet tests." To be sure, Otis has attempted by means of an ingenious chart to provide MA's and IQ's which are *estimates* of Binet MA's and IQ's that subjects *might* have earned when younger. But such estimated MA's and IQ's are neither psychologically nor statistically equivalent to Binet values, and the psychologist who uses these terms without proper explanation and qualification—as does Lorge—performs no service other than that of confusing and misleading his readers.

The IQ is a developmental concept, suitable for use with age-scales such as Stanford-Binet. The function of the IQ is to balance mental status against life age over the period when growth is progressing. Up to age 14–15 the IQ is useful and informative; after this period the MA no longer increases with CA and the IQ is of no value. To use the term IQ with reference to 34 year olds is indefensible and incorrect.

The IQ should be used only with tests so constructed that MA's derived from test scores will, when divided by CA, return a constant ratio. Requirements for a constant IQ are well known⁵ but perhaps may be profitably repeated here. In order to yield a constant IQ a mental test must (1) exhibit IQ distributions, the *SD*'s of which are equal at each age level (this means that *SD*'s of MA distributions must increase systematically with age); (2) measure the same abilities at successive age levels; (3) provide IQ's which show no consistent tendency either to increase or to decrease with CA.

The requirements for a constant IQ are *not* met by any group tests with which I am familiar. They are met by the Stanford-Binet. The term IQ, therefore, should *never* be used with group tests but should be confined strictly to tests of the Binet type. To be sure, some group tests⁶ keep the IQ approximately constant by assigning an IQ of 116, say, to the score in each age distribution one sigma above the mean; an IQ of 132 to the score two *SD* above the mean, etc. But such statistical IQ's are *not equivalent* to Stanford-Binet IQ's, and when used interchangeably confuse rather than inform. The Otis SA Test does not provide MA's and IQ's equivalent in meaning to the Binet values. Lorge's use of the terms MA and IQ, therefore, is unwarranted. And the implication from his study that 7–8 years of education will *raise* the IQ from 103 to 115 ("two full years") is without foundation in fact.

⁴ OTIS, A. S., *op. cit.*, p. 5.

⁵ MCNEMAR, QUINN. *The revision of the Stanford-Binet Scale*. New York: Houghton Mifflin, 1942, p. 155.

⁶ *Pintner General Ability Tests, Verbal Series*. Yonkers, N.Y.: World Book, 1939.

NOTE ON DUNLAP'S REMEDY FOR DEFECTIVE COLOR VISION

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It is a well-known fact that normal color vision is a requirement for qualification in certain industrial occupations. Because of the occurrence of some degree of defect in color vision in five to ten per cent of the male population, there has been a great incentive during the war years to discover some means by which otherwise eligible men in this group could be salvaged for military specialties. Diligent research sponsored by Army and Navy experts has failed to produce favorable evidence for any remedy for color blindness.

Among the various remedies tested was vitamin A. It appears that the credit for suggesting vitamin A for this purpose belongs to Dunlap and Loken (1). The general effect of the suggestion, in addition to stimulating some unsuccessful experimental attempts to confirm it, was to encourage the development of quick cures for men who wished to evade the draft by volunteering for officer training. For a time the number of such treatments constituted a small racket which was not easily curbed. (2)

While the problem of correcting defective color vision seems less urgent with the end of the war, the fact that Dunlap (3) persists in an elaborate defense of his original unsubstantiated notions on the effectiveness of vitamin A requires some examination.

In reading Dunlap's latest report one is reminded constantly of the difficulties of conducting research from an academic base on a shifting wartime population. The incidence of frustrated plans; the prevaricating, vagrant, and otherwise unreliable subjects; and the sudden induction of men on the threshold of a cure—all tend to make the reader of the report sympathetic. Under such difficulties it is not surprising that the results obtained were of little value.

Aside from some original and very novel speculations as to the nature, distribution, and possible causes of color blindness the only new material presented by Dunlap consists of 14 case histories of individuals who received vitamin A and cobra venom. In some cases perhaps vitamin B was given. The criterion tests were Loken's modification of the Nela test, and the Ishihara and American Optical Company pseudo-isochromatic plates.

In 13 of the 14 cases presented the treatment was not complete, according to the author's own statement. In the other case the conclusion was that the defect was irremediable. If the summary state-

ments given on each case may be interpreted literally, then Dunlap himself rejects 12 of the 14 cases as questionable. On the basis of results presented on tests before and after treatment he should have rejected 14 out of 14.

To fully appreciate the complete failure of these 14 cases to give support for vitamin A treatment it would be necessary to examine the data and to comment on each one, but detailed review here is not justified. Of the group as a whole Dunlap says: "While no conclusions could be drawn from these cases, their features suggest certain probabilities and agree with some of the notions which had occurred to us earlier."

As a check on Dunlap's original report that 80 per cent of his cases were able to pass chart tests after taking vitamin A, Elder (4) examined approximately 900 R.O.T.C. cadets at Louisiana State University. Of the 65 men showing various degrees of defective color vision, 41 completed the course of treatment. Results were entirely negative. The method used in this experiment closely paralleled that of Dunlap's even to the use of Ishihara and American Optical Company charts which he now condemns as unreliable but continues to use.

Dunlap suggests that pro-vitamins were used in the LSU experiment and that the units of vitamin A administered were unknown, presumably because the material was obtained from a questionable source. A careful reading of my report shows that "the material used was a vitamin A ester of high potency, determined spectrographically and confirmed by bio-assay" and that it was supplied by the Norwich Pharmacal Company. The implication that vitamin products of this company may be inferior to those of Squibb, Lilly, or Upjohn, indicates that Dunlap is unaware of Norwich research facilities and its systematic assay of all vitamin preparations.

If Dunlap persists in defending the notion that vitamin A has a beneficial effect on defective color vision, we are entitled to request that, in his next study, he observe the following conditions:

- a. Use only subjects with demonstrable defect (by any standard criterion he chooses).
- b. Leave nothing to be presumed as to whether subjects take their doses regularly and in prescribed amounts. Supervised administration is not difficult.
- c. Carry through *complete* treatment on at least a few cases so that there will be no need for guessing as to what might have happened if doses could have been continued for a few more weeks.
- d. Take necessary precautions against the possibility of learning the tests. (It is known that many men have passed service tests in this way. Confirmation of final test results with an additional test which the subject had not seen previously is desirable.)

It is impossible to abandon completely the hope that Dunlap's notions may lead to new knowledge of treatment for defective color vision. Until we have that knowledge in the form of thoroughly reliable observations, further discussion is superfluous and misleading.

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BOOK REVIEWS

LINTON, RALPH (Ed.) *The science of man in the world crisis*. New York: Columbia Univ. Press, 1945. Pp. xiv+532.

The science of man referred to in the title of this symposium is anthropology. The editor, himself an anthropologist, feels that the time is ripe for a synthesis of the sciences which deal with human beings and their problems. Though "anthropology is by no means the only discipline which has concerned itself with the study of man," he maintains that it holds a central position among these disciplines and that it should assume the responsibility for synthesizing their contributions.

The purpose of the particular synthesis attempted in this volume is to give people who are planning a more satisfactory world order a better understanding of the potentialities and limitations of the human material with which they are dealing and to provide them with techniques for controlling that material. Several chapters deal in a practical and helpful way with world resources, world population problems, and the administration of Indian and colonial affairs. One chapter shows how the results of the analyses of American radio audiences and programs might be helpful to a world government in its attempts at influencing public opinion.

Though there are several references to the need for a study of the biological man, no biologist is included among the 22 contributors. This is the more surprising since the anthropologists seem agreed that culture has no existence apart from the individuals who originate, transmit, and change it, and, therefore, that culture must bear the imprint of human nature.

Klineberg is the only psychologist contributing to this volume. His chapter on racial psychology is an excellent, carefully worded, brief survey and interpretation of psychological research in the area of racial differences.

In their discussions of cultural assimilation and change, several of the anthropologists lean heavily on learning theory as developed by the Yale Institute of Human Relations. Cultural changes are explained in terms of their capacity to relieve anxieties or to provide positive satisfactions. On the other hand, it is evident that some anthropologists do not find this theory adequate to explain all aspects of cultural transmission. The child seems to acquire some of the culture of his society in an automatic fashion in which it is difficult to discern any signs of either drive or reward.

One chapter reports a symposium on the concept of culture. The participants are several anthropologists, a psychologist, an historian, a lawyer, a physician, a psychiatrist, a philosopher, a business man, and an economist. Psychologists will find it revealing and chastening to get the anthropologist's reactions to their theories, approaches, and terminology.

It is evident from this book that modern anthropology is increasingly interested in the universals of human nature and of human cultures. One chapter, on the common denominators of culture, deals entirely with the surprisingly large number of activities, such as religion, faith healing, dancing, family living, incest taboos, fire making, and cooking, which are found in all cultures, past or present, throughout the world. Instead of attributing these universals in a facile fashion to instincts or even to simple biological needs, the author shows in a clear and penetrating fashion how they are the outcome of the complex interplay of these needs with secondary, acquired motives developed as a result of the world wide functioning of the principles of learning.

This book marks a welcome, albeit modest, advance toward an actual synthesis of the sciences which study man. Its principal value to psychologists will probably be (1) as evidence of the values to be derived from closer cooperation with the other biological sciences and (2) as a survey of the frontiers of research in anthropology proper.

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KARDINER, ABRAM, (with the collaboration of) LINTON, RALPH, DU BOIS, CORA, and WEST, JAMES. *The psychological frontiers of society*. New York: Columbia Univ. Press, 1945. Pp. xxiv+475.

Rarely are psychologists presented with a book more challenging in its social implications than this one. It is truly a cooperative endeavor, involving not only the skill of the authors named in the title above but also of Dr. Emil Oberholzer who analyzed independently, or without collusion, 38 Rorschach tests which had been administered by Du Bois to natives of Alor. The raw data were supplied by the collaborators. They consist of careful descriptions of three cultures, namely, the Comanche culture as described by Linton, the Alorese culture pictured by Du Bois, and the culture of an American rural community presented by West. Although the analyses and interpretations of these cultures, as well as the conclusions, are the work of Kardiner, there is evidence indicating a frequent interchange of ideas among all of the collaborators with the exception of the Rorschach specialist. The result is a well-coordinated book in spite of the diversity of its subject matter.

The concept which is indispensable for the interpretation of the different societies is *basic personality*. There can be no doubt that Linton in his *Foreword* and Kardiner in Chapter II, *The Technique of Psychodynamic Analysis*, but more especially in the main body of the book, have indicated the anatomy and dynamics of basic personality with greater clarity than they achieved in an earlier volume entitled, *The Individual and His Society*. Preferred to all others in deriving the characteristics of basic personality is the psychoanalytic technique.

Not, however, basic drives or "instincts" but action systems with identifiable perceptual, coordinative, and executive features are the fundamental constituents of basic personality. There is a generous departure from Freudian theory, a departure dictated somewhat by the anthropological data. It must not be assumed that Kardiner has approached his task with ready-made concepts, or that he has forced the data into preconceived molds. The nuclear constellations of basic personality, differing as they do from society to society, have been distilled from materials collected by the anthropologists. Kardiner, therefore, has not merely repeated the anthropological findings to append to them facile interpretations, rather he has painstakingly studied the data to derive his concepts from them and to uncover relationships not readily discernible to the original investigators. He has shown, for instance, how in infancy and early childhood the impact of maternal care, sibling interactions, paternal dominance or its absence, and of conduct more indirectly related to institutions, establish action systems which in their dynamic interactions yield basic personality. These action systems give rise to projective systems which later determine, for example, religion, folklore, and the arts. The projective systems are supplemented by reality systems which deal with explanations of the outer world and ways of dealing with it, or with conventionalized means of getting along with other people. Projective systems and reality systems together constitute basic personality but the former are both primary and dominant; in fact, the projective systems exercise a directing influence over the reality systems.

Kardiner does not stop with the derivation of the type of basic personality characterizing the three cultures studied intensively, instead he proceeds to demonstrate how the basic personality, in turn, explains many of the differences among the cultures and within each society. He has sought, in a measure, to deal also with the problem of variability among the members of a given society, a problem which was neglected in the earlier volume referred to above. In this task he derives considerable aid from biographies of persons in Alor and in *Plainville, U.S.A.*

The similarity of interpretation between analyses of personality made by Kardiner and by Oberholzer, who did the Rorschachs "blindly," is striking. At least this is so for what Kardiner calls the diagnostic features but not for the dynamics of behavior. Even though we are told there is no substitute for actually living with people, the correspondence in interpretation leaves us wondering which method of analysis contributes more to the validity of the other. Both methods combined suggest problems not likely to have been discovered by either one alone.

In the final or fourteenth chapter, Kardiner permits himself a flight of controlled imagination, a thought provoking and intriguing in-

terpretation of history in accordance with his concepts of basic personality. His discussion of the social significance of projective systems and the part they play in stabilizing the social order is packed with meaning for the psychologist and sociologist.

Only in the first chapter does the author appear to betray a slight aggressiveness not accounted for by the psychological techniques he condemns. Classical psychologies generally associated with the names of Wundt, Watson, Pavlov, and of Wertheimer, Köhler, and Koffka, are criticized because they render little or no aid in determining the dynamics of social behavior. Lewin's topology is dismissed in three sentences, or shall we say it is identified with psychoanalysis as an experimental handmaiden? Yet Kardiner throughout the book does not hesitate to write of circumstances conditioning the child one way rather than another. Had he desired to trace carefully and specifically how the earliest components of action systems were learned and integrated, I suspect his indebtedness to Pavlov would not be less than that owed to Freud. Psychoanalytic methods are not applicable to all aspects of experience and behavior, nor are the methods of any one of the current psychologies Kardiner has found wanting. Since psychoanalytic techniques and concepts enabled him to explain even the apparently unrelated forms of behavior, institutionalized and otherwise, he did not need to defend his preferred psychological system through a series of counter-attacks.

The book is not a text for undergraduate students, although the anthropological accounts will have an intrinsic appeal to them. A considerable knowledge of psychodynamics is required to follow and understand the analyses of the several societies. Graduate students in psychology, sociology, and anthropology, and especially their instructors, have been challenged to reexamine their basic concepts and interpretations of social behavior. They cannot afford to neglect a volume as richly suggestive as this one.

CHARLES BIRD.

University of Minnesota.

LAZARSFELD, P. F., BERELSON, B., & GAUDET, H. *The people's choice. How the voter makes up his mind in a presidential campaign.* New York: Duell, Sloan, & Pearce, 1944. Pp. vi+177.

This book is not a report of a routine political public opinion poll but is a cogent analysis of the factors that influence the voter in making his decision. As such, a careful reading will repay sociologists and political scientists as well as psychologists.

The first two sections lay the background by describing the people and social milieu of Erie County, Ohio, chosen as the sampling area because of its relative representativeness. Two sections are devoted to a discussion and interpretation of the data. Appendices refer the reader

to appropriate source material and elaborate certain technical items.

The data were gathered by eight waves of personal interviews obtained at one-month intervals just prior to the 1940 presidential election. Three control groups were used to determine whether the initial interviews with the main panel would make them "election conscious" and influence responses on subsequent questionnaires. Intensive repeat interviewing permitted a thorough study of voters' characteristics, but more important, allowed respondents' attitudes and opinions to be related to events of political significance soon after they occurred.

Social and cultural factors are demonstrated to be marked determinants of voters' preferences, and although people made up their minds to vote in a given way at different stages of the campaign, they tended to do so in terms of their political predispositions. The political campaign developed interest in the election for the indifferent, but did little converting of the partisan. Campaign arguments merely provided the latter with rationalizations to support his preferences. "Opinion leaders," who were more alert to events but more unresponsive to arguments opposing their predilections, were found to affect the mass of voters more than formal discussions in magazines and newspapers and on the radio.

The information presented regarding election behavior is in itself important, but in their ability to evoke meaning from the data, the authors made their greatest contribution. Fractionation and recombination of the data bring out pertinent relationships that would not be apparent from a simple tabulation of responses. Some will regret that the methods employed in gathering the data were not treated in greater detail, but few will deny that the use of scientific polling techniques as demonstrated in this book will make possible the addition of new concepts in social psychology.

LESTER GUEST.

The Pennsylvania State College.

LINDNER, R. M. *Rebel without a cause; the hypnoanalysis of a criminal psychopath*. New York: Grune & Stratton, 1944. Pp. ix+296.

For those who have never had first hand experience with psychoanalysis—nor read James T. Farrell—this book will be both interesting and educative. The suggestion of Sheldon and E. T. Glueck in their short introduction that the book is a significant milestone in criminology, however, is rather startling in view of the fact that it presents the results of only one case and uses a method that is by no means new in spite of the author's seeming claims in that direction.

Two hundred and fifty-nine of the 289 pages of text are the actual stenographic results of the psychoanalysis of Harold, who is psychiatrically diagnosed as a psychopathic personality and who is heavily

sentenced to the penitentiary for a serious crime, the nature of which is not disclosed. During the analysis Dr. Lindner meets the customary resistance from his subject and speeds its resolution with the aid of hypnotic recall, justifiably pointing out that this valuable therapeutic technique has been too cavalierly dismissed by analysts ever since The Master discovered it was not a panacea.

The conclusion of the author that his method has verified "the major but until now unproven hypotheses of the traditional psychoanalytic view of this entity" (psychopathic personality) seems presumptuous in view of the fact that (1) he has not been dealing with an entity, and (2) the logical meaning of proof is something more than restatement on the basis of one case. Yet the fact that hypnoanalysis as a method of diagnosis and treatment has great value is overwhelmingly evident even when the actual results are down in black and white and therefore robbed of some of their emotional force. That Dr. Lindner has skillfully conducted the treatment is also apparent, yet it will be this necessary skill combined with an admittedly rare *rapport* which will limit the widespread acceptance of this method for practical penological uses.

The first and second chapters discuss respectively the problem of criminal psychopathy and the author's method employed in its study. With the exception of a short summary chapter the remainder of the book is given over to the analysis record with occasional interpolated remarks by the author.

With a brilliant stroke of the technical *mot juste*, the author describes the definitive and classificatory role of psychopathic personality in the psychiatry of the past as a Pandora's Box. The force of the comparison for critical purposes, unfortunately, is somewhat clouded in his succeeding discussion when the psychopathic personality quickly becomes a "they" to which are attributed both general and specific typing characteristics. It is indeed strange to read on the first page of chapter one, for example, that this catch-all category has no accepted delimitation, and then on the last page of chapter two that the author's method has finally "penetrated to the core of psychopathic personality for the first time in the long history of psychological concern with this puzzling classification." Nowhere in the pages between or the ones that follow is there presented any definition of this "entity" which would give it a core to be penetrated.

The situation is further clouded by the facile use of analytic circularisms which, as usual, pose as explanations. Although the author recognizes that the statement: "The psychopathic personality super-ego is weak" is circular (he uses the words "self-explanatory"), he feels no dissatisfaction when he explains the acknowledged infantile behavior of the psychopath as "an abrupt cessation of psychosexual development." In addition to the possible multiplication of such logical

sterilities, the author can also be charged with illegitimate borrowing of what may well be principles of physiology but are certainly fictions at the level of total behavior. Cannon's canon of homeostasis is indeed well illustrated by the intricate nervous regulation of man's internal environment and in this connection serves as a big gun, but it is hardly permissible to say that the adjustive behavior of the neurotic is comparable to the range in *pH* content of the blood. Nor is it permissible, to the reviewer at any rate, to conclude even speculatively in favor of a structurally defective brain in psychopathic personality because the behavior of the latter is without restraint and the supposed function of higher nervous centers is to exercise restraint over lower centers.

The problem of the legitimacy of infantile memories is solved to the author's apparent satisfaction by comparing the motor reactions of his subject during hypnotic recall with the Gesell norms for child development. Harold's crucial six to eight months' memories are accepted as valid, but the author does not report whether or not at this time his subject was putting his own foot in his mouth.

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Indiana University.

ENGLE, T. L. *Psychology: principles and applications*. New York: World Book Co., 1945. Pp. ix+549.

This textbook was written for high school students who will probably take no further formal courses in psychology. It is a self-contained presentation and omits bibliographic references to other volumes or experimental literature. Accompanying the text is a *Teacher's Manual* containing some of these references and other material to facilitate the subject presentation. The author had extensive experience as a teacher of high school psychology before moving to the college level.

Throughout the text, the author prepares a factual and attitudinal foundation for students who will continue their exposure to psychology only through such "advanced 'texts' . . . (as) magazine articles, radio programs, motion pictures, sermons, and lectures; . . . (while) their psychological 'laboratories' will be the homes, the businesses, the clubs, and the communities in which they will function as citizens."

Most of the 17 chapters present subject matter of immediate interest and practical value to high school students. The following sample chapter headings illustrate the functional style which keynotes the entire volume: *Friendship and Love*; *Popularity and Leadership*; *Improving Learning Techniques*; *Getting in Touch with Our Environment*; *The Senses*; *Unusual Personalities*; and the final apt chapter heading, *Concerning Several "Mysterious" Matters* (hypnosis, dreams, etc.). However, there are three chapters which are less consistent with this central theme. Chapter I, *An Introduction to the Science of Psychology*, includes a discussion of the scientific method and a short statement of

the special fields of psychology. This material is, by necessity, highly abstract and would probably be more meaningful after the student has become familiar with some specific problems and procedures of psychological analysis. Chapter II, *Preparing to Read Psychology*, is a summary of basic statistical methods. The same criticism applies here particularly since far less use is made of statistical terms in the remaining chapters than is indicated by the emphasis given in this chapter. Both of these chapters have considerably less inherent interest to the high school student than the information in the rest of the book and it is unfortunate that this traditional, though questionable, introduction should be presented to students of this level.

The second of two chapters on learning is better than the first since it is an application of fact and theory to the learning-in-school situation while the first is a digest of the basic concept and experimental techniques used in both animal and human studies. Also included in the first chapter on learning is a well written discussion of thinking. This section might profitably have been expanded into a separate chapter.

This must have been a difficult text to write. Undoubtedly it is easier to list in summary form a series of related psychological studies than to interpret these findings in a way that will answer fundamental psychological questions confronting high school students. Many times the author must have wished that psychologists would or could find ways of examining the psychological attributes of more daily bread and butter problems. However, rather than be tempted into a series of student-interest lessons of psychology applied to daily life, the author consistently presents and interprets information drawn from scientific and critical source material.

This book includes a large number of illustrations and examples drawn from contemporary events—particularly material resulting from psychological and related scientific contributions developed during the war. As this information becomes more widely disseminated and continues to be released from military restrictions, the volume will acquire even greater practical applicability for high school students and teachers.

The reviewer recommends this textbook for examination by every teacher of high school psychology. This book should aid in making available to a large number of younger students the more practical results of psychological research and analysis.

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University of Arkansas.

BIRD, CHARLES & BIRD, DOROTHY M. *Learning more by effective study.* New York: D. Appleton-Century, 1945. Pp. viii+275.

This is another book on how to study. The fact that more than forty books and manuals on this subject have been published in the United

States since 1926 is indicative of the importance of the problem treated. It may also signify that individuals working in this field have not been entirely satisfied with the productions of their colleagues and co-workers. A major weakness of many of the available texts on how to study is that their numerous suggestions have been based largely on opinion rather than upon research in the field or upon sound principles of the psychology of learning.

This new book by the Birds, however, is refreshingly different in this respect. The procedures for effective study which they advocate all appear to be founded upon well validated psychological principles and theories which have come from the experimental literature. Instead of giving numerous conflicting and confusing rules for study, this book suggests a few highly significant methods and principles which have wide applicability. The authors, furthermore, appear to be concerned with the total personal adjustment of the student as well as with his academic achievements.

The book is not only psychologically sound in its approach, but it is conservative and accurate in its claims as to the amount of improvement that can be expected to result from special attempts to develop better study habits. For example, in discussing the progress made by college students who devoted eight weeks to the improvement of their speed of reading, the authors state that "they read 16.5 per cent faster in the final than in the initial test." (99) Although this finding is undoubtedly typical of the improvement one might expect to find in this particular skill during such a period of practice, it is quite unusual to find authors who do not make much more sweeping and extravagant claims.

On the whole, the book *Learning More by Effective Study* represents a very useful contribution to our knowledge of the psychology of study. College students, high-school seniors, and adults who will carefully follow its suggestions can with confidence expect to make substantial improvements in their study techniques.

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University of Illinois.

ABRAHAMSEN, DAVID *Men, mind and power*. New York: Columbia Univ. Press, 1945. Pp. viii+155.

A psychiatrist and refugee from Norway holds psychoanalytic post-mortems on the German nation, Hitler, Goering, Goebbels, Himmler, Quisling and Laval.

The people of Germany are diagnosed somewhat in Adlerian fashion as chronically suffering from feeling of inferiority with pronounced compensatory strivings for recognition. Their humiliation from defeat in World War I touched such a new low that temporarily they were at a complete loss for a way of compensation. Into this situation stepped

those arch-antinomians, Hitler, Goering, Goebbels and Himmler, acute sufferers from long-standing personal inferiority-feeling who thought they saw at once for themselves and the nation a way of salvation—a way to a thousand years of *pax Germanica*—in a projection of the causes for German misery upon the Jews, Communists and capitalistic nations and in a ruthless destruction or subjugation of these imaginary enemies. The Aryan population of Germany became as one in enthusiastic acceptance of this highly neurotic fiction which alone gave promise of a restoration of status and which was to eventuate in so much world-havoc. In anti-German countries, particularly France and Norway, elements of the population like Quisling, Laval et al., long smarting from frustration and non-appreciation, saw likewise a certain salvation for themselves, an opportunity for recognition in a throwing of their sympathies and activities with Hitler's new order against the interests of their own countrymen.

The latter part of the book is devoted to psychotherapeutic measures which Abrahamsen thinks should be taken by the United Nations in the interest of a permanent cure for this grand status-neurosis of the German people. Fully cognizant of the delicacy with which treatment must be undertaken by outsiders if the trouble is not to be exacerbated, the author indicates among others two rather significant approaches: 1. the replacement of men teachers in the schools by wholesome minded women; 2. a deführerizing of the father, i.e. a democratization of the German family set up.

The nuclear ideas of this book appear of interest to the general reader and most certainly provide food for thought for criminal and social psychologists. Unfortunately these ideas are somewhat obscured by the author's style which might be described as semi-free association.

F. C. SUMNER.

Howard University.

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Psychological Bulletin

The study of eye movements continues to be an important and much used technique for investigating the reading process. An overall summary of the field up to 1935 is given by Tinker (108). The last review (107) to appear in this journal was published in 1936. In the present review, material appearing during the years of 1935 through 1944 is considered. Bibliographies, critical evaluations, summaries of parts of the field, and textbook discussions will be found in references (13, 37, 38, 41, 74, 95, 97, 103, 116, 117). A few titles not available to the reviewer have been included in the bibliography for the sake of completeness (6, 29, 30, 46, 47, 48, 49, 126). Certain material on eye movements will not be reviewed since it is not closely related to the reading process. This includes reports on eye movements in viewing pictures and advertisements, certain studies of visual fixation, eye movements as related to visual attention, and the like.

The studies to be reviewed here group themselves as follows: techniques of measurement, analysis of the reading process, oculomotor coordination, factors in variability, special reading situations, heredity and certain environmental factors, eye movements in the reading clinic, training eye movements, and central versus peripheral processes.

TECHNIQUES OF MEASUREMENT

Apparatus. Although some of the new apparatus for recording eye movements consists of modifications of earlier techniques, others involve new principles. Brandt (9) describes an adaptation of the Dodge corneal reflection type of camera. Its special feature is simultaneous recording of the vertical and horizontal components of eye movements. A portable model of the Dodge type of camera, the Ophthalm-o-graph, is described by Taylor (105). Records of horizontal eye movements of both eyes are recorded simultaneously. Tiffin and Fairbanks (106) have developed a camera for simultaneously recording eye movements and voice reactions in oral reading. Miles' (70) note on the first eye-move-

ment record photographed by Dodge has historical interest. A demonstration film on eye movements in reading has been prepared by Valentine, Troyer and Brown (118).

An electrical method of recording eye movements is finding some use. Changes in the corneo-retinal potentials are closely proportional to the sine of the angle of rotation of the eye. With proper placement of electrodes and with an amplifying system, the movements may be recorded mechanically or photographically. Miles (71) and Halstead (40) suggest that eye movements in a variety of situations may be measured by this technique. Its validity for recording eye movements in reading has been checked by Hoffman, Wellman and Carmichael (43). Electrical and corneal reflection records were made simultaneously while their subjects read and while they made horizontal and vertical eye movements in viewing a dot pattern. The only data given for the reading responses was a sample record. Although the two techniques yielded similar records, they were far from identical. Where extent of eye movements and duration of fixation pauses for viewing the dot patterns were measured, the agreement as shown by correlations is close but again the records do not yield identical results. Although this electrical technique has sufficient validity to justify its use in certain situations where it is impossible or inconvenient to photograph the corneal reflection, it is clear to the reviewer that it is not satisfactory in its present form for studying eye movements in reading.

Another electrical technique, recording photographically (electromyograms) the action currents generated by movement of extrinsic eye muscles during reading, has been used by Luckiesh (61) and Luckiesh and Moss (62, 63, 64, 65). The sample record given by Luckiesh (61, p. 128) is either not typical or the author has read the time intervals wrongly. He gives 0.120 sec. for time of back sweep when the typical value is approximately 0.050 sec. The duration of his interfixation movement also appears unduly long. Furthermore, the demarkation between successive fixation pauses is not always clear. A direct comparison of this technique with the corneal reflection method is needed to check the validity of the former. The corneal reflection technique of photographically recording eye movements, therefore, remains the most satisfactory method for use in reading investigations.

Reliability. The reliability or consistency of eye-movement records has been considered by several workers. Imus, Rothney and Bear (44), using fifty word samples of text, found that eye-movement measures had low reliability ($r = .59$ to $.72$). In another report Imus, Rothney and Bear (45) re-emphasize the unreliability of eye-movement records.

Employing the same material, i.e., that furnished with the Ophthalm-o-graph, Anderson (3), and Broom (12) note similar trends in low reliability. In a more extensive study Tinker (112) found that reliability of eye-movement measures was low only when short samples of text (5 to 15 lines) were employed. When 20 to 40 lines were read the reliabilities were about .80. Thus eye-movement records will have adequate reliability only when a satisfactorily long sample of reading is used. When only group comparisons are involved, short samples are adequate. But in the clinic, where individual diagnosis occurs, records from reading at least 20 lines of text are indicated. An even longer sample is desirable.

Validity. If eye-movement measures are to have meaning for validity evaluation there must be adequate comprehension of the material read. Users of the Ophthalm-o-graph for photographing eye movements have commonly employed short samples of reading material with questions on comprehension furnished with the apparatus. Imus, Rothney and Bear (45) have found this check on comprehension to be useless. Authors such as Broom (12), Anderson (1, 3) and Imus, Rothney and Bear (44, 45), have reported very low validity coefficients for eye-movement measures of reading when correlating eye-movement measures with scores on reading tests. Such a comparison constitutes an unsatisfactory test of validity since it is well known that reading scores derived from different reading situations are not related or yield only low correlations. Tinker (112) has shown that when the material read before the camera is strictly comparable to that in the performance tests the validity of fixation frequency and perception time (fixation frequency times pause duration) was very high ($r = -.80$ to $-.99$). Pause duration by itself was found not to be a valid measure of reading performance.

Other evaluation. Is a reader able to give a typical reading performance in the apparently artificial laboratory situation where the photographing of eye movements takes place? Tinker (112) has checked this and found that the performance was the same before the camera as when working at a table. This finding has been confirmed by Gilbert and Gilbert (34). It should be noted, however, that it is important to adapt a reader to the laboratory situation when photographing eye movements.

Stromberg (101) has questioned the common practice of plotting the fixation points of the eyes to show the location of these fixations on the material read. This has ordinarily been done for a single eye, assuming that the two eyes coordinate in fixating on the text. When Stromberg

plotted the fixations for both eyes he found what appear to be startling discrepancies in binocular fixation. That is, the fixation of one eye frequently did not coincide with that of the other eye. Undoubtedly there has been unwarranted optimism concerning the usefulness of plotted fixations to show exactly where the eyes paused. Since one fixates an area rather than a point, and because movement of the reflected beam does not correspond exactly to the visual angle of movement, plotted fixations can only represent approximations to the correct place of fixation. One suspects that part of the binocular fixation discrepancy found by Stromberg was due to the reflected spot of light not being in corresponding locations on the two eyeballs, a condition which would prevent correspondence on the 'photographic record. Nevertheless, this study does raise an important question concerning interpretation of plotted fixations.

In general the evidence indicates that under adequate experimental conditions, eye-movement records are reliable and valid measures of reading performance.

ANALYSIS OF THE READING PROCESS

The fixational pause. Perception occurs during fixational pauses. Arnold and Tinker (5) made an extensive analysis of the fixational pause of the eyes. On the average a pause of .157 sec. was required to identify a letter and .172 sec. to accurately fixate a dot. These are longer than the .100 sec. required for a well cleared-up perception. In a variety of reading situations the mean pause duration varied from .217 to .404 sec. These are much longer than the pauses made in simple fixations requiring a minimum of perception. The longer durations in reading may be explained in terms of the requirements for comprehension and assimilation of the materials read. Luckiesch and Moss' (65) statement that reading pauses average about .150 sec. is a misrepresentation. Reading with comprehension cannot have a mean pause duration as short as this.

An excellent analysis of regressive pauses, i.e., pauses following backward eye movements within a line of print, in the reading of 9th and 10th grade children is given by Bayle (7). She found that regressive pauses tended to fall into six patterns: adjustment after the first fixation in a line, adjustment within a line when the span of vision is overreached in a too long forward move, regressions for verification, regressions during word analysis, regressions for phrase analysis, and regressions for re-examination of a whole line. It was found that regressions were caused by need to complete perception and by difficulties of

interpretation due to failure to recognize the meaning of a word or failure to connect meaning of a word with the context. Bayle points out that regressive pauses are necessary parts of the reading process in analytical reading. Buswell (14) gives a cautious approval of this view but many writers ignore it. Recognition of the fact that regressions are legitimate and essential in certain kinds of reading would avoid some of the misconceptions involved in training eye movements (see p. 110).

The fixation span. This is the amount of material read in one fixation. The larger the span, the fewer the fixations and consequently the faster the reading. By using reading material with spaces between phrases and encouraging readers to fixate each phrase only once, Robinson (87) obtained the relation between perceptual span from tachistoscopically exposed material with fixation span in reading. For tachistoscopic material not in context this correlation was .52, for material in context (using a special tachistoscope) the correlation was .66. He concluded correctly that span derived from short exposures predicts eye-movement behavior inadequately. La Grone (55) and Knehr (51) also found a low correlation between perceptual span and fixation span. Dearborn and Anderson (27) have perfected a highly flexible and effective motion picture technique for exposing successive phrases in context to increase the fixation span.

In a study of adult reading, Buswell (14) emphasizes that fixation span or span of recognition is an important index of reading maturity, i.e., a narrow span indicates immature reading habits. He found no relation between visual efficiency and span. There is a similar emphasis upon fixation span in the author's second monograph (15). In all of Buswell's work (14, 15) there is a sound emphasis upon training to increase the span of recognition which directs attention to perception rather than to the motor aspects of eye movements.

Luckiesh and Moss (62) have studied the influence of typographical factors on extent of the fixation span. They found that span decreased slightly in going from four to 10 point type and that it increased slightly in going from 13 to 21 and 29 pica line widths. Their interpretations reveal a decided lack of understanding of the reading process. No account is taken of possible accompanying changes in either pause duration or efficiency of peripheral vision.

OCULOMOTOR COORDINATION

Rhythm. Much has been made of rhythm in eye movements. Buswell (14) states that "a mature reader covers a line of print with regular,

rhythmic eye movements" but admits that reading habits should be flexible and that in some situations regressions are desirable. Taylor (105) has devised techniques for developing "rhythmical, left-to-right eye-movements." In the non-experimental literature the importance of rhythmical eye movements is stressed, i.e., regular forward movements along a line of print and about the same number of fixations from line to line are considered highly desirable. This emphasis is derived from the notion that the efficient reader is efficient because he is able to establish rhythmical eye-movement habits more readily and that favorable typography fosters rhythmical movements. Sisson (94) has made a quantitative analysis of motor habits of eye movements in reading as related to length of the printed line, kind of material read, and level of reading ability. Tendency toward rhythmical motor habits was evaluated by means of a habit index, i.e., the percentage of successive lines having the same or nearly the same number of fixations. The habit index was higher for reading the shorter lines of print and slightly higher for the more efficient readers. This apparent relation is accounted for in terms of probability, however, rather than a habit for the fewer the fixations, the better the chance of two adjacent lines having the same number of fixations. The habit index favored neither the reading of easy narrative nor scientific prose. Another alleged sign of rhythmic eye movements or *short-lined motor habits* as they are sometimes called, is a characteristic temporal distribution of pause durations which is alleged to be fostered by shorter lines. Although some evidence for such a pattern was found, it was no different in short than in long lines of print. In view of his results, Sisson suggests that the concept of *short-lined motor habits* is useless. The reviewer would add that this notion of rhythmic reading is not only a useless concept but a harmful one. For years the view that rhythmical eye-movement behavior is characteristic of effective reading and is highly desirable has directed attention toward eye movements. This has led to an undue emphasis upon peripheral oculomotor mechanics to the sacrifice of adequate attention to the more important central factors of comprehension and assimilation (see *training of eye movements* p. 110). The findings of Walker (120) are in line with the above conclusions. He found "no evidence of a habit of moving the eyes any fixed distance or in any rhythm" in his analysis of eye movements of good readers. If rhythm is absent in the eye movements of highly skilled readers, as indicated in these studies, the view that rhythmical eye-movement patterns are desirable for effective reading becomes meaningless. Weaver (123) found a high degree of

variability in ocular behavior characteristic of both memorizing and recall.

Motor efficiency. Tinker (111) has ascertained the relation of oculomotor efficiency to reading proficiency. There were no significant correlations between reading proficiency and either accuracy of visual fixation measured in terms of number and extent of corrective moves at fixation point or speed of convergence-divergence moves. Only when extremes of the group were compared was there a slight relationship evident. These findings refute previously held views. Similarly, Strombert (99) found no differences between fast and slow readers in convergence and divergence movements during reading. Knehr (51) noted that vergence movements in reading monocularly were the same as for binocular reading.

Eye-movement time. The proportion of time taken by interfixation movements in reading is relatively small and varies with kind of material read. Tinker (109) found that the per cent of reading time devoted to moves ranged from 5.3 to 9.6 with an average of 7.3. The more careful and analytical the reading, the smaller the relative time taken by moves. Luckiesh and Moss (63), after noting that there is no clear vision during horizontal eye movements in reading, state, "However, this unique and advantageous arrangement is stated to be absent in the case of vertical movements of the eyes." There is no sound basis for the latter statement.

Monocular and binocular vision. Apparently oculomotor behavior is about the same whether one or both eyes are used in reading. Knehr (51), employing eye-movement measures, found no significant differences in performance between monocular and binocular reading. Furthermore, the oculomotor patterns were alike under the two conditions. Clark (23) found that alternating vision which occurs during bar-reading, did not appreciably alter reading efficiency as measured by eye-movement records. Further experimenting with a larger group of subjects and with a more adequate sample of reading is needed to check these results. Nevertheless there is evidence to support the above findings. A mass of data indicates that central factors of comprehension and assimilation rather than peripheral, i.e., mechanical factors, are dominant in determining reading proficiency.

FACTORS IN VARIABILITY

Variation in subject matter. For some time it has been recognized that variation in subject matter produces changes in eye movements.

Additional information on the nature of the variation is contained in the following studies. Seibert (89) made an analytical study of eye movements of 8th grade pupils while reading mathematics, biography, adventure, physical science, history and geography. Differences in all eye-movement measures occurred in going from one subject matter to another, but these differences are not as significant as the author appears to believe. The critical ratios ranged from .00 to 2.53. It is admitted that some children maintained a definite pattern of eye movements throughout all materials. This indicates a lack of flexibility in reading habits. A similar analytical investigation was completed by Stone (98) with college freshmen as subjects. They read arithmetic, biology, English, educational psychology, physical science and social science. Although the critical ratios are not large, differences that tended toward significance were obtained for eye-movement measures in going from one kind of material to another. Implications of the findings were not discussed. Both of these studies may be criticised for faulty statistical interpretations.

Condition of reader. This category is concerned with such factors as maturity level, reader proficiency, stuttering and deafness. Buswell (14) has investigated the eye movements of adult readers. Measures employed for diagnosing reading maturity include:

1. The span of recognition or amount of print recognized in a single pause depends directly upon fixation frequency. A relatively wide span indicates greater maturity.
2. Rhythmic regularity of progress of eye movements along the line of print is considered symptomatic of a high degree of maturity in reading.
3. For any given kind of reading the more mature reader will make fewer regressive movements than the immature reader.
4. The more mature reader employs eye-movement pauses of briefer duration.

On all these measures the adult readers tended to be less mature than high school seniors. In fact the adults were only slightly better than 6th grade children in reading material within the comprehension range of both children and adults. Buswell emphasizes that reading habits should be flexible, i.e., they should adjust themselves to the kind of material read. Nevertheless, he failed to note that in general his adult readers revealed a marked lack of flexibility of oculomotor behavior in going from easy to more difficult material.

Anderson (2) compared the oculomotor patterns of 50 good and 50 poor readers to find how such readers adjust to changes in the difficulty of the reading material, and to reading for different purposes. All eye-movement measures distinguished good from poor readers. For both

groups, fixation frequency, regression frequency and pause duration increased with the more difficult reading material. Good readers showed a greater flexibility in eye movements in adjusting to increasingly difficult material. Greatest irregularity in oculomotor patterns occurred in reading to remember details. Here the differences between groups was present but less pronounced. It was concluded that two important determinants of regularity in eye movements are difficulty of material read and purpose of reading. In a qualitative analysis of eye movements of good readers, Walker (120) discovered that on the average fixations were distributed evenly throughout the line for both easy and difficult material. Pause duration was greater in the first part of lines. About 80 per cent of lines were covered by fixations and the last fixation was nearer the end of the line than the first. Extent of forward shift of eyes was not related to duration of fixation at end of the shift.

There is not complete agreement concerning the oculomotor patterns of stutterers. While Moser (73) found that eye movements in silent reading are less effective for stutterers than for normal children, Hamilton (42) found no differences. The former considers that stuttering may cause reading disability while the latter states that stutterers do not need special treatment in the silent reading program. Since Hamilton's experiment was more adequately controlled, the more acceptable view is that eye movements in silent reading are the same for both stutterers and normal readers. In Moser's (73) study, silent fixation was the same for stutterers and normal speakers, but when stutterers talked during fixation, a variety of eye movements occurred. During oral reading, differences in oculomotor patterns were associated with stuttering spasms. These findings suggest that extrinsic eye muscles as well as other muscle groups are not under control during stuttering. Working with another kind of handicapped group, La Grone (56) found that the eye-movement patterns of deaf children differed from those of hearing children. This was especially true for developmental levels in reading. Although pause duration tended to decrease from grade to grade, fixation and regression frequency increased and then decreased. The most common kind of regression occurred at the beginning of lines, but there were fewer regressions for deaf than for hearing children. Since the groups studied were very small, these findings need checking.

Oral versus silent reading. A few studies have been concerned with eye movements in oral reading. Buswell (14) points out that schools have recognized that the processes of oral reading and of silent reading are psychologically different. Experimental work yields evidence on the

degree of this difference. The eye movements of good, poor and unselected college subjects during silent reading were compared with eye movements during oral reading by Anderson and Swanson (4). The correlations for the eye-movement measures ranged from .50 to .70. Coefficients for the poor readers were somewhat higher. In silent reading there were fewer fixations and regressions and shorter pause durations. Going from poor to good readers, the differences in eye movements between oral and silent reading increased. Thus the eye-movement patterns tended to be more similar in the two kinds of reading for poor readers. These data show, therefore, that there are common elements in oral and silent reading, especially among poor readers. According to Buswell (14) silent reading of adults differs from oral reading in respect to the degree that vocalization is suppressed. Evidence from eye-movement measures while silently reading material designed to exaggerate vocalization supports this view. Under these conditions the eye movements of poor readers were like movements in oral reading; those of good readers were not.

In studying the eye movements of adults during oral reading, Buswell (14) found that the mean eye-voice span is an excellent index of the maturity of a person's oral reading habits. The greater the span the more mature the reading. An excellent study of eye movements of good and poor readers during oral reading is reported by Fairbanks (31). Both eye and voice records were made. As in the above study the eye-voice span reflected maturity of reading habits. Although difficulty of textual material determined variations in eye-movement measures, the good readers were less affected than the poor readers. No fixed pattern of eye movements was discovered. Analysis of regressions in relation to eye-voice span together with a comparison of the eye-movement patterns of good and poor readers led to the conclusion that faulty eye movements do not cause errors in reading but that the errors are central in origin.

Miscellaneous. Certain studies are not easily classified under the above groupings. Simpson (92) found no significant correlation between eye-movement measures and time spent in leisure reading. It is concluded without justification that eye movements are not improved unless specifically trained. In another study, Simpson (91) obtained correlations of $-.38$ to $-.52$ between fixation and regression frequency versus college achievement and mental ability. Pause duration showed no appreciable correlation with achievement and intelligence. Reading test scores correlated higher than eye-movement measures with achieve-

ment and intelligence. The inference that eye-movement training is desirable because the correlations are low is a misinterpretation of the data (see *training eye movements* p. 110). Imus, Rothney, and Bear (45) found that the correlations for eye-movement measures versus college aptitude, scholastic achievement and reading test scores were only .07 to .38. Their conclusion that eye-movement measures are unreliable and invalid is not justified (see *reliability* p. 94 and *validity* p. 95). In a similar study with children, Leavell and Sterling (58) found low to moderate correlations between eye-movement measures and intelligence. Taylor (105) obtained significant differences between eye-movement measures of children making normal school progress and of those failing. The grade norms given by Taylor are of doubtful value because an unreliable sample of eye movements was obtained. In fact Taylor was largely responsible for providing the inadequate samples of reading material employed by subsequent users of the Ophthalm-o-graph. They are too short to yield reliable records.

Peripheral vision. For years writers have been emphasizing the importance of peripheral vision in reading. Cues perceived in peripheral vision not only furnish premonitions of coming words and phrases, but they also guide the movements of the eyes to new fixations along the lines of print. La Grone (55) has studied the relationship between peripheral vision and eye movements in reading. Accuracy of perception in the left and in the right peripheral fields was obtained from tachistoscopic exposure. High perception scores in the left peripheral field tended to go with fast reading, few fixations and shorter pause duration. But high perception scores in the right peripheral field tended to go with slow reading, more fixations and longer pause duration. Regression frequency was not significantly related to perception scores. Higher perception scores either in right or left peripheral fields are said to be due to preference. Since vision in the right peripheral field is supposed to be important in reading, why do the more able readers have better perception to the left? The author misses the point that in tachistoscopically exposed material the long practiced habit of reading material from left to right is dominant. It is logical to infer that such a habit is more effective for good than for poor readers. Furthermore, a short exposure test of peripheral vision is not an adequate measure of peripheral vision as it operates in the normal reading situation where the successive fields of vision overlap. While the findings in this study are interesting, they do not provide a crucial test of the role played by peripheral vision in normal reading.

SPECIAL READING SITUATIONS

Spelling. In a follow-up of an earlier study on spelling (see 107), Gilbert (33) obtained records for six children over a three-year period. The eye movements revealed growth characteristics in perceptual habits. Although there was little change in pause duration, fixation and regression frequency decreased and there was evidence of a more effective method of attack in studying the words. The more mature spellers differentiated between hard and easy sections of words and their ocular responses tended toward simple verification reactions. Gilbert and Gilbert (35) recorded eye movements before and after training for speed and accuracy of perception in learning to spell with 4th, 5th, and 6th grade pupils. Training resulted in less perception time per word and fewer fixations and regressions, but no change in pause duration. Thus the training resulted in a larger fixation span and less attention to details, but the general pattern of cross study and accuracy of spelling was maintained. Here is a good example of how training in perception habits with no attention to eye movements yields changed oculomotor patterns which reflect more effective central processes of apprehension and assimilation. In a later study, Gilbert and Gilbert (36) found that spelling is learned more effectively when words are studied in isolation than when encountered as critical words in reading. Eye movements revealed that when words were in text the reading was disrupted, especially for poor spellers. The authors' suggestion that a concentration of fixations may represent spelling reactions rather than comprehension or recognition difficulties would hold only if the readers were set to learn spelling as they were in this experiment due to a prior spelling test.

Reading music. The reading of music is a highly specialized oculomotor task which involves both vertical and horizontal eye movements. Lowery (60) notes that individual differences are in terms of pause duration rather than fixation frequency. Weaver (124) obtained photographic records of eye movements and of keyboard performance for 15 trained musicians during sight reading and playing of three selections at a piano. From one to two notes were read per fixation. A musical note is considered equal to a word in reading. Pause durations are longer in music reading than in word reading and vary with the complexity of note relations. The author's correlations of .82 to .89 between pause duration and reading time and of only .27 to .58 between fixation frequency and reading time are in contrast to word reading where fixation frequency yields the high correlations. This supports Lowery's (60) finding that pause durations provide the individual variation. According to Weaver (124), predominance of horizontal or of

vertical progression of eye movements depends upon the nature of the music. Notes on the treble staff are usually but not always read ahead of notes on the bass staff. The eye-hand span was highly variable. Regressions appeared to be related to kind of music read. In another study, Van Nuys and Weaver (119) investigated eye movements of 12 experienced musicians while reading musical rhythms and melodies of 12-note phrases, each presented for a period of 2.8 seconds. Memory span equalled the number of notes correctly played for each phrase exposed. Regressions occurred more for the melodies than for rhythms while pause duration increased with increase in complexity of note relations for both melodies and rhythms. Pause duration tended to be longer for rhythms than for melodies and there were more notes correctly played per fixation for the rhythms. Findings in these studies of eye movements in reading music furnish material for a comparison with the reading of words:

1. Considering a note comparable to a word, the fixation span is similar in both kinds of reading:
2. Regressions serve somewhat the same purpose in both situations.
3. Increase in complexity of text brings increase in complexity of eye-movement patterns for both music and words.
4. Reading music involves both vertical and horizontal eye movements while reading words in text involves only the latter. There are of course minor vertical deviations of the eye in reading a line of print. And the return sweep to the next line involving both vertical and horizontal displacement, is common to both situations.
5. In reading words, fixation frequency is the best indicator of reading rate but in music pause duration is best.

HEREDITARY AND CERTAIN ENVIRONMENTAL FACTORS

Twin studies. Two studies of twin similarities in eye-movement patterns while reading prose have been reported. Morgan (72), comparing eye-movement measures for members of pairs in a well controlled experiment, found very low correlations (.04 to .24) between artificial pairs, higher (.24 to .53) for fraternal twins, and still higher (.61 to .72) for identical twins. These coefficients indicate more fundamental bases for individual differences in eye movements in reading than is furnished by a "habit" concept. The "underlying processes of assimilation" constitute a more logical explanation of the results. Hereditary factors play a significant role to the extent shown by the correlations. It is noteworthy that pause duration, which is least susceptible to training, showed the highest correlations for twins. The author correctly infers that, in treating poor reading, consideration should be given to capabil-

ities of the individual rather than exclusively to certain ideal motor sequences characteristic of oculomotor patterns of efficient readers. In a second study, Jones and Morgan (50) report additional evidence on twin similarities in eye-movement patterns. Median correlations for eye-movement measures in reading revealed a trend similar to that in the earlier study: artificial pairs, $r = .105$; fraternal twins, $r = .435$; and identical twins, $r = .530$. Three judges had considerable success in identifying on the basis of general appearance the eye-movement photographs of identical twin pairs when they were mixed with photographs from fraternal twins. Considering the many factors which may modify oculomotor patterns, it is significant that this identification was better than chance. These results, therefore, support the earlier conclusion that eye-movement patterns in reading are not determined wholly by training. Genetic factors are involved to an appreciable degree.

Emotion. Emotional disturbances appear to modify eye-movement patterns. Strongin, Bull, and Korchin (102) report that when readers expect a shock, visual efficiency in terms of eye movements decreased by about 10 per cent and binocular coordination became worse in 14 per cent of readers. Warren and Jones (122) obtained eye-movement records while subjects read in the laboratory and while they were exposed in a high place. No significant differences in fixation and regression frequency or pause duration were found. But unsteadiness of fixations appeared when in the high place, especially for acrophobic subjects.

Illumination. Luckiesh (61) and Luckiesh and Moss (64, 65) report the effect of variation in illumination on eye movements in reading. Records were obtained at the beginning and at the end of one hour of reading under one foot-candle and again under 100 foot-candles of light. Speed of reading was slowed up significantly greater under the one foot-candle of illumination in comparison with the 100 foot-candles. The difference was due largely to changes in pause duration. Although the authors condemn speed of reading as a measure of readability of print, they employ eye movements for this purpose, apparently unaware that eye-movement measures are speed of reading measures.

Readability. Eye movements have been used extensively by others to determine legibility or readability of print. Bell (8) found no difference in eye movements for reading typewriting versus manuscript writing, but a significant difference in favor of typewriting versus cursive, and in favor of manuscript versus cursive writing. Differences were mainly due to changes in fixation frequency rather than pause duration. Tinker and Paterson have completed eight eye-movement studies on the readability of type. In a long series of studies (78) these

authors had compared speed of reading for various typographical variations. Significant differences were found in certain comparisons. The analytical eye-movement studies were undertaken to discover the specific patterns of eye movements related to the disclosed differences. The non-optimal text, which was read slower than the more readable material, was characterized by the following changes in eye-movement patterns:

1. All capitals (slower) versus lower case—more fixations (113).
2. Old English (slower) versus a modern type face—more fixations (114).
3. Ten point type: 19 versus nine pica (slower) line widths—more fixations and longer pause duration; 19 versus 43 pica (slower) line widths—more fixations and regressions plus longer pause duration (79).
4. Six point type: 13 versus five pica (slower) line widths—more fixations, fewer regressions and longer pause duration; 13 versus 36 pica (slower) line widths—more regressions and longer pause duration (81).
5. Type size: 10 versus six point (slower)—more fixations and longer pause duration; 10 versus 14 point (slower)—more fixations and longer pause duration (80).
6. Type sizes in optimal line widths: 11 versus eight point (slower)—more fixations and longer pause duration; 11 versus six point (slower)—longer pause duration (82).
7. Black print on white background versus red print on dark green background (slower)—more fixations and regressions plus longer pause duration (115).
8. Several variations: optimal versus non-optimal (slower) typography—more fixations and regressions plus longer pause duration (83).

These analytical studies of eye movements in relation to typographical variations indicate that non-optimal conditions may only increase fixations or pause duration, or both of these. Regressions may increase or decrease. Factors operating to change the oculomotor patterns include changes in visibility (small print, low contrast between print and background), in word form (large print, all capitals), prevention of optimal use of peripheral vision (very long and very short lines), or lack of familiarity with a type face (Old English).

Visual fatigue. Eye movements, along with other measures, have been employed in the search for adequate measures of visual fatigue. Dearborn (26) presents some tentative data which suggests that eye movements may reveal visual fatigue. After a long day of visual work consisting of proofreading and study, speed of reading was reduced and saccadic movements through relatively long angles were retarded. Since only two readers served as subjects, these trends need checking. According to Clark and Warren (25), during a 65-hour vigil there was no consistent trend in variation of fixations, regressions or pause duration

for eye movements in reading. Although sporadic and temporary changes in binocular behavior occurred for certain individuals, there were no uniform trends as the period of wakefulness increased. Since compensation is possible, eye movements apparently failed to furnish an adequate picture of fatigue effects. Kurtz (54), however, claims that eye-movement records reveal fatigue after 30 minutes of severe visual exertion. It is difficult to evaluate his data as presented. The most thorough evaluation of eye movements in reading as measures of visual fatigue has been made by McFarland, Holway and Hurvich (69). Thirty minutes of either rapid saccadic exercise or 30 minutes of rapid convergence-divergence movements failed to produce any significant changes in eye movements while reading. Although their results were negative, the authors consider that eye movements ought to provide a sensitive indicator of visual fatigue if a more sensitive recorder were available. Such an apparatus would yield a more precise measure of fixation and of binocular coordination. But fixation and regression frequency and pause duration in reading would remain unchanged.

In general these studies reveal that eye movements in reading are insensitive indicators of visual fatigue. Only when the fatiguing task is extremely severe is there a suggestion that eye movements are modified. Two factors are probably preventing fatigue from affecting eye movements in reading: (a) All experimenters have used only 10 to 12 lines of text to be read before the camera. The visual mechanism can compensate enough to give normal performance for the short periods of time required to read such materials. (b) Furthermore, there is no adequate way to evaluate precisely the degree of comprehension in the short paragraphs of text ordinarily used. The reader, since there is a tendency to maintain one's oculomotor pattern, may read two paragraphs with similar eye movements but with different degrees of comprehension. An adequate check of eye movements as measures of fatigue can be obtained only with long samples of reading text equated for difficulty and so constructed that comprehension is satisfactorily measured or maintained constant.

Anoxemia. Definite and consistent modification of eye movements in reading occurs with anoxemia. McFarland, Knehr, and Berens (67, 68) found significant increases in reading time, i.e., perception time plus eye-movement time, and fixation frequency in oxygen concentrations corresponding to 15,000 and 18,000 feet. Of greater importance, perhaps, was the qualitative variation of ocular movements in oxygen want. That is, nystagmoid tendencies and general unsteadiness appeared and there was an accentuation of abnormalities such as muscle imbalance.

Furthermore, latent oculomotor defects frequently became apparent indicating that the procedure may have clinical significance. In general, altered oculomotor behavior yielded a sensitive measure of the early effects of anoxemia.

EYE MOVEMENTS IN THE READING CLINIC

Views concerning the usefulness of eye-movement records in the reading clinic vary. Kurtz (53) is probably over optimistic when he claims that the importance of a permanent objective record, i.e., eye movements, can not be over-estimated. The view of Clark (21) that eye-movement photography serves as a valuable objective auxiliary to other techniques is more moderate. Opposed to these is the view of Imus, Rothney and Bear (44, 45) who, in appraising eye movement photography as a clinical method, are convinced that the technique lacks both reliability and validity. (See criticism of this view under *reliability* p. 94 and *validity* p. 95.) Tinker (110), in an evaluation of eye-movement measures, notes purposes for which they may be used. He concludes, however, that eye-movement records are not indispensable in clinical diagnosis of reading status. An exaggerated stress is placed upon the study of eye movements in the clinical situation by Hamilton (41) and Sievers and Brown (90).

Orthoptic training. Peters (84) found that training with the Binocular Synchronizer and the Squint Korrektor yielded a reduction in fixation and regression frequency. These changes, however, were similar to those produced by motivated reading practice alone. More extensive studies of this kind are reported by Parkins (75, 76, 77). Apparently there was marked improvement in reading with corresponding changes in eye movements due to orthoptic training. In this group of studies, the results are so presented that it is difficult to evaluate them. The reviewer is inclined to reserve judgment until check studies are made.

Visual abnormalities. Extensive clinical use of eye-movement records has been made by Clark. In an initial study (17), comparing the eye movements of individuals having normal binocular balance with those having muscular imbalance (exophoria), revealed no differences in either fixation frequency, regression frequency, pause duration, or time required to complete divergence movements at the beginning of the lines of print. For similar comparisons in later reports (18, 19, 22), Clark also found no difference in fixation and regression frequency but there were differences in vergence movements during reading. The exophoric readers made significantly greater divergence movements at the beginning of lines and required more time to complete these corrective divergence

movements. It is suggested that the large divergence movements of subjects with muscular imbalance may cause enough fatigue to be important in remedial reading programs. Clark (20) reports similar trends for a case of high exophoria. After noting that his experiments reveal no striking differences in oculomotor patterns between normal readers and those with muscular imbalance, Clark (24) concludes correctly that such deficiencies are apt to cause undue fatigue and should be corrected. His belief, however, that visual deficiencies cause reading disability in spite of the negative evidence is not well taken. In a correlational study, Stromberg (100) found no relation between amount of vergence movements and phoria of eye muscles. This, however, does not negate Clark's contention that it is desirable to correct muscle imbalance for the individual case.

Remedial reading. In an extensive study of remedial reading at the college and adult levels, Buswell (15) employed the eye-movement camera to measure with precision the changes in perceptual habits which occurred during the experiment. He states, however, that an eye-movement camera should be considered a research rather than a clinical instrument. Simpler methods of measurement which are entirely satisfactory in an ordinary remedial program are available. Once a procedure in the reading clinic has been validated by eye-movement records, there is no need to recheck the procedure in other remedial groups.

TRAINING EYE MOVEMENTS

Problem. The early experimenters were concerned with identifying and describing oculomotor behavior during reading. As this field of investigation developed, differentiating factors in eye-movement patterns were more emphasized. That is, stress was placed upon developmental variation, differences between good and poor readers, variation with changes in subject matter read, etc. This eventually led to the practice of attempting to improve reading performance by training eye movements. The necessary assumption is that eye movements are important determinants of reading proficiency. Thus, if the retarded reader is trained to use eye-movement patterns similar to those which characterize efficient reading, his reading proficiency would presumably improve. This emphasis upon the mechanics of eye movements tended to direct attention to peripheral factors as determinants of reading performance rather than to the central or underlying processes or perception, apprehension and assimilation.

Methods. Techniques of training eye movements in reading vary. On the one hand are those, represented by Pressey (see Tinker, 108) and Sievers and Brown (90), who try to train subjects to pace eye fixations at three regular intervals along a line of print. It is true, however, that Sievers and Brown suggest that eye-movement training is but one small part of a reading program. Nevertheless it is naïve to assume that if a line of print is separated into three groups of words, that paced eye movements will result in reading each group with one fixation. In fact, Dearborn and Anderson (27) have demonstrated that such an arrangement results in more than three fixations. Buswell (15) also notes that few subjects ever achieve three fixations per line. At the other extreme are those who attempt to increase the fixation span by employing techniques which approximate the normal reading situation. Thus, Dearborn and Anderson (27) have devised a flexible film projection method for teaching phrasing to increase the size of fixation span. Approximately the same principle is involved in the still-film projector method developed by Buswell (15) to increase span of recognition or fixation span. Successive phrases of text are exposed in proper sequence and spacing to lead the eye along in normal perceptual sequences. Timing and nature of textual material are flexible in both techniques. Between these extremes a variety of techniques have been employed to train eye movements. The most commonly used one is the metron-o-scope designed to develop "controlled reading." It is described by Taylor (105). This is a mechanically operated machine for exposing each line of print in three successive sections. Taylor (105) states that the fundamental idea of the metron-o-scope is to develop the mechanical, i.e. eye movement, and interpretative processes simultaneously. As noted above, such techniques do not reduce fixations to three per line.

Controlled reading. A large number of reports describe training of eye movements or controlled reading. Since Anderson (1, 3) Sisson (95), Kottmeyer (52), Nelson (74), Pilant (85) and Traxler (117) have evaluated or reviewed much of this material, detailed notice to the separate studies will not be given here. The experimental reports on controlled reading include studies by Broom (10, 11), Dearborn and Anderson (27), Dodson (28) Garver and Matthews (32), Hallock (39), Lauer, Henry and Fritz (57), Lee (59), Robinson (86), Simpson (93), Taylor (105), and Witzeman (125). Although some of these studies were done without adequate controls and lacked acceptable statistical treatment, they are rather uniform in showing considerable improvement in reading due to the training. Three studies in this area will be surveyed in more detail.

Sisson, (96), using matched groups of adult readers found that reading with intent to improve produced significant changes in eye movements comparable to the changes from eye-movement pacing. In a more elaborate study, Cason (16) employing matched groups of school children, found no significant differences in improvement in reading for the students trained in phrase reading or in eye movements by use of the metron-o-scope in comparison with those who did motivated library reading. That is, free library reading resulted in the same gains as programs stressing the oculomotor mechanics of reading. All made significant gains. Buswell (15) employed a film projector for controlled practice to increase the fixation span or span of recognition. This resulted in improved reading proficiency which was reflected in changed oculomotor patterns. Emphasis was upon apprehension of broader perceptual units so that the reader would be freed from the requirements of detailed visual perception. This permitted greater attention to meanings.

Evaluation. Eye-movement training may be evaluated as follows:

1. Many so-called procedures for training eye movements or for controlled reading result in improved reading efficiency.
2. The improved reading status is reflected in modified oculomotor patterns.
3. The improvement obtained by eye-movement training, with or without elaborate apparatus, is no greater than that resulting from motivated reading alone.
4. There is no adequate evidence that training eye movements as such improve reading (107). Experiments concerned with pacing eye movements and controlled reading usually involve other techniques and are never divorced from increased motivation. Buswell (15) flatly states "training eye movements does not increase reading ability."
5. The training of eye movements too often becomes a ritual and tends toward an overemphasis upon oculomotor mechanics to the sacrifice of adequate attention to the more important processes of perception, apprehension and assimilation. This training may result in a decrease in the flexibility and adaptability of reading habits which characterize good readers. According to Buswell (15), "The exploiting of machines and gadgets" to control reading "by persons who do not understand the psychology of reading seems at present to be adding greatly to this mechanistic folly." And Traxler (117) has "difficulty in seeing any justification for purely mechanical attempts to train pupils in better eye movement habits."
6. The reviewer agrees with Traxler (117) that controlled reading techniques, i.e., training eye movements, "need not be used in a purely mechanical way but that they might well be used to supplement, in a way that would have strong motivating force," a program in which reading comprehension is emphasized. The reviewer believes, however, that as long as gadgets are used by those with an inadequate understanding of the psychology of reading, we will continue to have the undesirable emphasis upon oculomotor mechanics.

CENTRAL VERSUS PERIPHERAL PROCESSES

Problem. Training of eye movements is intimately related to the problem of whether eye movements are causes or symptoms of reading proficiency. The view that eye-movements are causes of reading proficiency is based upon the discovery that when there is ineffective reading, there are many fixations and regressions and that good readers use few fixations and relatively few regressions. This trend developed until the point was reached where it was claimed that the good reader is good because he uses few fixations and rarely makes a regression. Thus, to develop a good reader presumably it is only necessary to develop effective oculomotor habits. This gradually led to a strong emphasis upon peripheral factors, especially among the large group of writers with an inadequate knowledge of the psychology of reading. A few writers, including this reviewer (107, 108, 111) began to question this emphasis. It was pointed out that the more important determinants of reading proficiency were the central processes of perception, apprehension and assimilation. These processes are reflected in oculomotor patterns because eye-movement habits are very flexible and appear to adjust themselves readily to any change in the perceptual processes involved in reading. It would appear that eye movements merely reflect efficient or poor reading performance rather than cause it.

Recent evidence and conclusions. There is really no evidence to support the view that eye movements determine reading proficiency. As shown in the above analysis, the results obtained from training eye movements do not support the view that peripheral factors are dominant although based upon the assumption that they are. On the other hand, there are numerous reports which indicate that the central processes are the important determinants in reading performance. Analysis in which the writers conclude that eye movements reflect the central processes of perception, apprehension and assimilation include: Anderson (1, 3), Traxler (117), and Sisson (95, 97). A variety of recent experimental studies of eye movements have led to the conclusion that oculomotor patterns reflect poor and efficient reading, not cause it. These include: Anderson (2), Fairbanks (31), Knehr (51), Rogers (88), Sisson (94), Swanson and Tiffin (104), and Walker (120), and Walker and Molish (121). For earlier studies see Tinker (107, 108). Changes in eye-movement patterns occur when difficulties of comprehension arise, with variation in reading purpose or attitude etc. It is now well established that oculomotor reactions are exceedingly flexible and quickly reflect any variation in the central processes of perception, apprehension and assimilation.

Evaluation. Careful consideration of the field would probably indicate that there need be no conflict in assigning proper roles to peripheral and central factors in reading. To read, one must perceive words in proper sequence and to do this the eye fixations must progress in appropriate patterns. In other words, oculomotor patterns are a means of achieving the perceptual sequences necessary for apprehension and assimilation of the textual material. The particular eye-movement pattern employed is conditioned by the nature of the central processes. Clear apprehension and rapid assimilation of the textual material is reflected in relatively few fixations and regressions. But ineffective perception and confused apprehension, accompanied by difficulties in assimilation, characteristically produce many fixations and regressions. The latter, however, should not be confused with the oculomotor pattern occurring in highly analytical reading where it is necessary to re-examine the text and to work out relationships. In such a case, as shown by Tinker, Frandsen and others (see Tinker, 107), the complex eye-movement pattern may indicate effective reading rather than inefficient central processes. It seems possible, therefore, that the unfortunate dichotomy of central versus peripheral processes might be avoided to some degree by omitting use of the terms eye-movement patterns and rhythmical eye movements, and by employing instead the concept of perceptual sequences. The latter is more meaningful in discussing the reading process and it avoids the unfortunate mechanical implications of the former.

SUMMARY STATEMENT

Important progress in the study of eye movements in reading has been made during the past ten years. Many of the investigations have been of high quality. Nevertheless unfortunate trends have appeared. Many unqualified individuals, those without an adequate background in either experimental procedures or the psychology of reading have been doing "research" in eye movements. The results are deplorable. Furthermore, the exploitation of the eye-movement technique has accentuated the unfortunate emphasis upon peripheral determinants of reading proficiency and has led to wide use of eye-movement measurement in the clinical situation although the eye-movement camera should be considered a research rather than a clinical instrument. There are signs, however, that the conclusions from sound research and the views derived from adequate analysis of results in the field are beginning to make headway.

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COLOR ADAPTATION TO 1945*

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It is the purpose of this paper to present a critical history of the phenomenon known to workers in color as color adaptation, color fatigue, or color minuthesis.

Although usually attributed to later investigations, the first scientific observation of color adaptation appears to be that described by the French mathematician and physicist De La Hire (12, p. 289) in 1694. His was a description of observations of the sun, the course of perceptual change, and the resulting after image. He ventured an hypothesis concerning the fatigue of visual receptors, and although it is somewhat erroneous in view of later investigations, it is characteristically the Hering theory.

The first important investigation was the 1863 contribution of Maria Bokowa (5). Bokowa wore copper oxide glasses of 1 mm. thickness from four to five hours. The glasses had a transmission which was predominantly red, but they could not have been highly selective since she was able to see colors in all parts of the spectrum. The experiment was controlled, in a manner, since a spectrum was viewed from time to time during the course of adaptation and comparisons made from one observation to another. Further, there was an attempt at quantification by viewing a color wheel and describing the perceptual color which corresponded to a physical stimulus resulting from an arbitrary number of circular degrees of arbitrary "primaries." In general, Bokowa's results may be summarized as follows:

1. After color adaptation had taken place, all colors appeared as either yellow or blue.
2. The brightest color was a mixture of yellow and blue.
3. The cut off point of the visible spectrum shifted toward the short end of the spectrum.

It is to be noted that nowhere does Bokowa indicate that the adaptation phenomena continued to a neutral gray although Helson and Judd (19, p. 383) refer to Bokowa as having reached complete adaptation. Bokowa (5, p. 163) refers to red as appearing less intensive during adaptation and this indicates that some change in intensity may have taken place. She may, however, have been referring to a loss in satura-

* From a doctoral dissertation *The Color Adaptation of the Human Eye* directed by Professor H. P. Weld. For an additional paper based on the same dissertation, see Cohen (11).

tion although the word saturation does not appear in the paper. Bokowa, then, reports changes in hue, and makes references to changes in intensity.

In 1865, the phenomena was noted by Aubert (3) in his observations of red and blue colored squares against achromatic backgrounds. The loss of saturation was referred to as (3, p. 131) "Adaptation der Netzhaut für Farben." He adds that adaptation for blue light is faster than adaptation for red light. It is interesting to note that Sheppard (33, p. 35) says, "Nowhere in his writings does Aubert connect this term [adaptation] with color-changes, for which he uses the term 'fatigue'." The use of the term adaptation for both white and colored light apparently did originate with Aubert.

Sigmund Exner's experiment (14) in 1868 was the first which used a spectrum as adapting and reacting stimuli. His experimental procedure was to throw a spectrum on a screen. Slits were placed in the screen so that the light of the adapting color came through one slit while the light of the reacting color came through the other. Exner was, of course, studying the effect of the after image as well. By observing at a distance the beam of light which penetrated the first slit, he adapted a small area of the retina. The adaptation time was ten seconds. The eye was then brought very close to the second slit which had the effect of stimulating a much larger area of the retina. And in this larger area there appeared the after image of the light stimulus of the first slit. Inspection of Exner's table shows the following results:

1. When the stimulus color and the adapting color are identical, the adapted portion of the retina is always lower in saturation than the non-adapted portion.
2. The adapted portion of the retina perceived all colors as red, green, or blue.

With respect to this second result, Exner attempted to locate the spectral position of these basic colors whose hue does not shift during adaptation and toward which all other hues shift. For this purpose he devised an instrument utilizing a telescope and a spectrum. His results are as follows:

1. For red the position is from the long end of the visible spectrum to a point between C (656 $m\mu$) and D (589 $m\mu$), perhaps somewhat closer to C.
2. For green the location is between E (527 $m\mu$) and b (517 $m\mu$).
3. For blue it is in the immediate environment of the line G (431 $m\mu$).

One of the few early studies directly concerned with the changes in intensity during color adaptation was performed by Schön (31) in 1874. Schön was not interested in adaptation phenomena as such but rather in the physiological mechanism to which discrepancies among previous

experimenters who had determined the differential sensitivity of the retina to the different portions of the spectrum could be attributed. Schön suggested that the differences between the many investigators might be due to the color adaptation of the retina.

Schön used prisms to obtain spectral stimuli. These stimuli were presented to an observer through a tube which was divided in half forming a stimulus and comparison field. The intensity of each half was controlled by a variable slit, the width of the slit being a measure of the intensity. The experiment was begun by making the slits equal on both halves of the tube. The comparison field was shielded from the view of the monocular observer while the stimulus was presented for 3, 5, 10, or 15 seconds. At the end of the interval the comparison field was presented to the subject who stated whether they were equal in intensity. If they were not judged to be equal, the slit of the comparison field was changed and the determinations continued until they were judged to be equal. Three stimulus colors were used—red, green, and blue. Schön came to these conclusions:

1. After five seconds exposure to spectral red, green or blue, one half of the brightness is lost.
2. There is a further decrease in brightness which takes place gradually.
3. This further decrease is greatest for blue and least for red.

Many criticisms have been leveled at this experimental procedure and consequently the results. His two test fields were not strictly comparable because of the construction of the apparatus. He ignored the differential sensitivity of the retina to various wave lengths in comparisons of heterogeneous wave lengths. For other criticisms and an alternative procedure which avoids most of the defects, see Almack (2, p. 27 ff).

Ssamujlow's 1889 experiment is only reported in abstract form (34). So meager is the information available that it is difficult to determine the exact nature of his investigation. Ssamujlow considered the duration of the after image as a measure of the adaptation of the retina, a theory which may not be strictly true. Ssamujlow does not adequately explain what he means by adaptation, but presumably he refers to loss in saturation. Using spectral light sources, he arrived at the following results:

1. The retina does not adapt equal amounts for different lights. It is most for red, less for green, and still less for blue.
2. The degree of the adaptation is dependent on the time and the strength of the stimulus.
3. The peripheral retina adapts to a lesser degree than the fovea.

There are two contributions to the literature by Carl Hess. The first (20), published in 1890, describes his experimental procedure. A telescope was arranged in conjunction with spectroscopes so that there appeared in the telescope a left and right field, either of which could be illuminated with monochromatic light. The field on the left was a square while the field on the right had the form of a circle. The telescope could be shifted so that either the right or left field came into view. The observer's eye was placed at the telescope and fixated the right edge of the left square field. After a given interval the telescope was shifted to the right field with the cross hairs in the center of the circle. Thus only one half of the adapted retina is covered with the new fixation, so that changes which occurred by virtue of the adaptation process are apparent by comparison of the fields. Nine different homogeneous lights and two non-spectral lights made by mixing spectral red and spectral violet were used as both adaptation and reaction stimuli. Changes in hue were noted for all combinations of lights taken two at a time; each light was used as an adaptation light and matched with all other lights as reaction stimuli. There were two adaptation times—10 seconds and 35 seconds.

The experiment was continued in 1893 (21) and here the apparatus was modified to give results in terms of wave length. The spectroscope was fitted with two adjustable slits; through one slit passed the adapting light while the reacting light passed through the other. The other spectroscope was fitted with a single adjustable slit. The telescope was not kept in a fixed position. The left half of the telescopic field could be filled with either the reacting light or the stimulus light while the right field was filled with light from the adjustable single slit of the right spectroscope. The observer fixated a point between the two fields for 50, 70, 75, or 180 seconds. The left field was illuminated with the stimulus light while the right field showed blackness. At end of the adaptation time the stimulus light was shut off and the reacting light and the comparison light presented simultaneously. The comparison light was readjusted after successive trials until it matched the perception of the left field. Hess reaches these conclusions:

1. There is a tendency for colors to appear as either blue, green, or red to the color adapted eye. This is only a tendency and the data present an erratic appearance. Hess' cursory attempt to locate the wave lengths of these colors using only three colors as stimuli (having theoretical implications in the Helmholtz theory) found them to be 490 $m\mu$ for blue, 565 $m\mu$ for green, and 650 $m\mu$ for red. These figures are quoted erroneously by Almack (2, p. 26) and Sheppard (34, p. 55 ff). To be sure, the basic colors are not well identified, but the data do indicate a red basic.

2. The shift in hue increases with time. There is no reference to saturation in this paper.

Voeste's 1898 experiment (45) is one of the keystones in the history of color adaptation. His apparatus was the Helmholtz color mixer (18, p. 395 ff) as described by König and Dieterici (22). Essentially two fields were presented to the subject. The observer monocularly fixated a point between the left stimulus field and the right comparison field. The stimulus field was illuminated with a monochromatic source while the comparison field was unilluminated. After ten seconds, the observer matched the right field with what was seen on the left. It was matched for hue by variation of the prismatic surfaces and for intensity by variation of the slit openings, while saturation was completely disregarded. The stimulus fields covered the visible spectrum at three or four different levels of intensity. Voeste's results may be summarized as follows:

1. All colors tend to shift in hue toward three basic hues, 560 $m\mu$, 494–498 $m\mu$, and 460–470 $m\mu$. Subsequent experiments using these wave lengths as test patches show no shifts in hue. Troland suggests that these changes are due only to the Bezold-Brücke effect (42, p. 210 or 43, p. 180).

2. The comparison field is from 18.9% to 83.3% of the intensity of the stimulus field. These figures are in terms of slit openings and, of course, fail to take account of the differential sensitivity of the retina.

In 1899 and 1900 Burch wrote two papers on the production of artificial color blindness. The results of the experiments described in these papers are so unusual that extreme caution needs be exercised in interpretation. In the first paper (8, p. 2) Burch wrote, "[The method] consists of exposing the eye to bright sunlight in the focus of a burning glass behind a transparent screen of the proper colour, and keeping it there until all sensation of that particular colour is lost." This must mean, it appears, that adaptation goes to a neutral gray. Burch continues (8, p. 3), "With a combination of ruby glass . . . it takes from a few seconds to two or three minutes to produce complete red blindness." It is not clear as to whether he means the perception was of red or of neutral gray. He does claim that for an eye stimulated as described above, the sensation of red is not present, and the eye is completely blind to that hue. If this is the case, adaptation not only reaches a neutral gray, but the eye remains in that state for some time since its perception of red would be similar to its perception of gray. This condition, Burch continues, does not in any way affect the perception of other colors having no red component. It has no effect on intensity at all.

In his second paper (9) Burch attempted to show that the effect was not pathological since it could be produced by moonlight. He wrote (9, p. 217), "In producing red-blindness, as there are no colors to the left of red, I have generally used a part of the spectrum which excites only the red sensation, and which therefore must continue to appear visible at all. But I have not thought it desirable to push the fatigue of the retina far enough to destroy this sensation of light." Of course, the spectrum could appear as gray and still be visible.

Beck (4) observed the effects of intense illumination on colors and noticed the adaptation effect. He continued his experiments with spectral sources and reported that the colors were not visible as hues or were desaturated, depending on the degree of adaptation.

Porter and Edridge-Green (29) adapted the eye with a band of color for twenty seconds. Immediately after the adaptation period the eye was shifted to view a spectrum larger than the band; the band filled only the center portion of the spectrum so that comparison could be made between the adapted and unadapted portions. Although the experimental procedure is interesting, their results are erratic and their experiments cursory. They noted that the retina did not lose sensitivity for red, but sensitivity was lost for blue. In general their results follow no general form or law and there is no direct statement pertaining to the law of color adaptation.

In 1913, however, Edridge-Green (13) contributed an additional series of experiments of more significance, two of which we report:

I. After the eyes have been exposed to sodium light for twenty minutes, a spectrum is examined. The yellow has completely disappeared from the spectrum; the green and red meet without any intermediate color. But the red, orange, and green have not lost any of the yellowness which they previously possessed.

II. Blue-green spectacles were worn for ten minutes. At first all white objects appeared as a vivid blue-green, but at the end of ten minutes' time, a white object appears white. On examining the spectrum with these glasses before adaptation, there was no red to be seen, there were small amounts of orange, and the blue, green, yellow, and violet were visible. After adaptation, there were no marked changes in the orange or any other color, except the green which took on a paler and more yellow appearance. The region of the spectrum corresponding to the dominant region of the transmission of the glasses was most affected by adaptation, while the blue and yellow on each side appear bluer and yellower respectively.

The conclusions follow:

1. No color is seen by the color adapted eye unless the corresponding physical stimuli are present in the light reaching the retina. Color adaptation pro-

duces its effect by subtraction, and not by the addition of any new color sensation.

2. Color adaptation increases with time.

3. The dominant wave length corresponding to the adaptation stimulus color of spectral regions appears colorless. Colors immediately on either side of the dominant wave length are shifted higher and lower respectively.

The experimental apparatus used by Brückner (7) was Helmholtz' *Spiegelapparat* (18, p. 161). This was essentially a piece of plateglass placed perpendicular to the table. The observer is able to look through the glass to the surface of the table beyond it, or to see the reflection of the near surface of the table, or both, depending on how the glass is covered. Brückner placed an adapting stimulus composed of two parts—one half chromatic and the other half achromatic—at the far end of the table. All of the colors were from the Hering series. The chromatic colors covered the visible spectrum and are indicated in Ångstrom units. The chromatic half was either white, gray, or black. Each hue appeared three times—once with each of the achromatic colors. The reacting stimulus, placed on the near table, was one of the seven hues used as one half of the adapting stimulus or one of six additional colors. The experiments were carried out binocularly using daylight. The individual fixated the adapting stimulus through the plate glass. After ten to fifteen seconds the apparatus was adjusted so that he observed the reacting stimulus as a reflection. The results are expressed in terms of whether or not the reacting stimulus is more effected by the previous adaptation to the chromatic or achromatic stimuli. Since Brückner did not report for the case when the reaction color was the same as the stimulus color, no conclusion may be drawn except that changes in saturation do occur.

In 1920 Hubert Sheppard (33) made the first systematic investigation of color adaptation. His paper presents four main experiments:

I. Five observers who fixated in direct sunlight seven large sheets of Hering colored paper which covered the range of the visible spectrum, described the course of adaptation during fixation. In general, they report either no hue change or a hue change to yellow, with no change in intensity. The loss in saturation proceeded to a neutral gray in from 45.9 seconds to 220 seconds. When the subjects observed 180° of the same colors mixed with 180° of black and white on a color wheel, complete adaptation took place in from 23.4 seconds to 121.3 seconds.

II. In the second experiment, circles of the Hering papers placed in a field of Hering gray were illuminated by a 75 watt bulb. By means of a telegraph key attached to a kymograph the observers signaled when important perceptual changes occurred and also when uncontrolled eye movements occurred. They did not give a running account of the

adaptation process. In general, adaptation was not characterized by shifts in hue as in the first experiment nor was complete adaptation reported by all subjects. All observers, however, reported loss in saturation and told of a cloud-like fog which moved over the color. The adaptation times (times at which no further change occurred) ranged from 40.3 seconds to 120.2 seconds. Sheppard determined the chromatic limens for each of the stimulus hues by the method of limits using a disc containing black, white, and the hue. The limen has a negative correlation with the adaptation time. Wherever the adaptation time is long, the chromatic limen is small.

III. In this series a remodeled spectrophotometer so designed that the subject, observing through an artificial pupil, was able to perceive a small portion of the spectrum as a disc, was used. Five positions were chosen from the visible spectrum. All but a single observer reported complete adaptation. The time ranged from 76.6 seconds to 169 seconds. Since Sheppard noticed that these times formed a curve which closely resembled the brightness curves for spectral colors, he performed another series in which the colors were equated for brightness value. The resulting figures, although more similar than those obtained before the brightness values were equated, still showed another factor to be operating. This, Sheppard reasoned, was the chroma value already demonstrated as being related to the adaptation time in his second experiment.

An additional series was conducted in which the chroma values were made equal (by subjective techniques). It does not appear that the brightness values were kept constant as well. His apparatus was too crude to control both factors at the same time. The adaptation times (for complete adaptation) for the various colors were now close enough so that Sheppard considered chroma as being the chief determiner of adaptation times. Introspective reports indicated no hue change except for red and violet which shifted toward yellow and blue respectively. There was no reported change in intensity.

IV. The last experiment was an attempt to determine whether complete adaptation times could be reached with very high intensities. A carbon arc provided the light source while gelatin filters were placed over a pair of glasses. The experiment was monocular. The subject fixated the arc while wearing the glasses until no further change took place. He then fixated a spectroscope which was equipped with a powerful illuminating source and a rheostat to control the intensity. (The rheostat no doubt changed the color temperature.) This served as a comparison light. The subjects reported complete adaptation in from 105.3 seconds to 197 seconds. There was some shift in hue toward yellow but most subjects reported no shift in hue at all. As Almack points out (2, p. 20 ff), Sheppard's description of this portion of his experiment is not clear. It is difficult to determine the nature of this comparison light formed by the spectroscope.

Troland (41) has reported the results of what he terms photopic adaptation. These are the same experiments referred to in his previous papers (37, 39, 40). The eleven experiments will be described:

I. In the first experiment, designed to test the equilibrium sensation for direct sunlight on white paper, a large white paper was fixated in direct sunlight for ten minutes. He concluded that prolonged exposure of the retina to large, very intense, achromatic stimuli does not involve a reduction of the sensation to neutral gray. The luminosity of the perception after adaptation is much greater than that of mid-gray. Further, the equilibrium sensation just described is brought about rapidly.

II. In this experiment large sheets of colored paper, selected to cover the range of the visible spectrum, were observed in direct sunlight. From these experiments he drew the conclusions that after exposure of the retina to large chromatic stimuli of high intensity, the resulting perception is not one in which the hue of the stimulus has completely disappeared. There may be some shift in hue but the perception is never one of neutral gray. The equilibrium point is reached soon after fixation.

III. A dark gray square, placed in the center of a yellowish-green Hering paper, was fixated until the green component of the paper faded leaving a light blue. At the end of four minutes a shutter arrangement substituted a Hering violet paper for the gray square. The outlying field became a darker blue while there was no reappearance of the green. But on the introduction of a white object into the green adapted area, the green color returned. Additional tests indicate that this phenomenon is not due to the contraction of the pupil. The experiment, of course, lends support to the theory of central control of color adaptation.

IV. Observations similar to those of Experiment I were made with stimuli consisting of two juxtaposed Hering papers of different hues. One paper filled the upper half of the field while the other filled the lower half. The six possible combinations of the four *Urfarben* were employed. Troland concluded that the principle of simultaneous color contrast applies to the equilibrium position. The introduction of a neutral object into the bi-chromatic field brings about the same change in quality for both halves of the field which was described in the previous experiment for a simpler stimulus. An experiment using a bipartite fixation of black and white failed to show any tendency of the field to approach neutral gray.

V. This experiment concerns the perception of a small luminous spot on a dark background. If the illumination is low, the spot tends to disappear and remain below the visibility threshold or fluctuate between visibility and invisibility. As Marbe (25) demonstrated, the more intense the spot, the less is the tendency for it to disappear. This was confirmed by Troland, and with spots of colored light fixated *monocularly* at high intensities, observers reported that the stimulus colors

changed as in the previous experiments, but it did not approach a neutral gray nor did the spot fluctuate.

VI. The fluctuations of the last experiment are explained by supporters of the Hering theory as being due to eye movements permitting the retina to recover. See, for example, Ferree (15). A large sheet of Hering paper was fixated. The paper was large enough to be well out of the field of possible eye movements. With steady monocular fixations, repeated fluctuations in the field could be observed. Temporary disappearance could be obtained.

Fluctuations described here cannot be explained on the basis of adaptation followed by recovery due to eye movement. Deviations in fixation would move the image on the retina through not much more than one fiftieth of its diameter. Changes in accommodation could not be responsible for fluctuations since such changes would only be on the edge of the image.

VII. This experiment was designed to investigate whether the changes in the iris can be responsible for the visual fluctuations which the previous experiment showed were not due to eye movements or accommodation. The stimulus surface described in the previous experiment was perforated and a telescope placed behind. The iris of the observer was illuminated so that changes could be observed through the telescope. The observer pressed a telegraph key when the colored spot was seen to disappear. The pupillary contractions and expansions have a perfect correspondence with the reported fluctuations.

VIII. If changes of the iris are responsible for fluctuative disappearances, it is obvious that the elimination of these changes by the use of drugs such as pilocarpin or atropin or the use of an artificial pupil should cause the elimination of these fluctuations. Drugs have been used on similar problems by Pace (28) and McDougall (27). Both investigators report that fluctuations were present. For the effects of the artificial pupil, see Troland (38). The intensity of the colored spot stimuli was placed under the control of the subject. Starting with a very low intensity, he increased the brightness until no fluctuations occurred for three minutes. The stimulus was then regarded as being at the required threshold. The results indicate that if the action of the iris is eliminated, no fluctuations occur for long intervals. The fluctuations which do occur, according to Troland, are due to mechanical difficulties of the artificial pupil, to the electrical condition of the visual system, binocular rivalry, etc.

IX. Spots of various colors equated for brightness were fixated with the artificial pupil. Since the brightnesses were of moderate intensity, no fluctuations of the spot stimuli were observed. The adaptation did not proceed to a neutral gray. If the stimulus is a psychological primary, there is no essential shift in hue. Intermediate hues tend to lose the red and green components.

X. The subject observed a colored spot for twenty seconds. The intensity of the spot was then decreased until the subject indicated that it had disappeared. Fixation was kept constant by means of the bright crescents appearing at the edges of the adapted area. Reappearance of the whole area always occurred.

XI. In the greater number of visual studies it is necessary to stimulate but a single eye while the other is blindfolded. This is an ideal condition for binocular rivalry. To demonstrate this phenomenon, one eye observed a colored spot. Immediately after fixation, the stimulus was transferred to the other eye where it appeared at first in its proper color. But soon it became a sparkling white, reminding one of binocular luster. It is therefore suggested that the after image appeared in the other eye and when mixed with its complement (the stimulus hue), it appeared white.

Troland did not discover this phenomenon. It was first noted by Newton in a letter to Locke and is the subject of Titchener's thesis. See Titchener (35) or for a brief discussion in English (36 p. 49 ff). See also Franz (16, p. 44) for evidence that this effect does not occur.

Troland and Langford reported in abstract form some experiments concerning chromatic minuthesis (44). Minuthesis is their term for adaptation. They contend that changes which occur are only changes in saturation. Any further changes in hue are due to the Bezold-Brücke effect.

In 1923 Hamilton and Laurens (17) used a comparison technique to study adaptation. One eye was fatigued for thirty seconds. Then both eyes were placed at telescopes which presented a colored field to each eye. The fields were adjusted until a just noticeable hue discrimination was indicated. The results were as follows:

1. Red, green, and blue have a selective effect when used as stimulating sources. All other colors do not.
2. During adaptation brightness is reduced.

One of the more important studies of chromatic adaptation was performed by Almack (2) in 1928. Her problem was to measure the loss of sensitivity of the color-adapted eye. Spectrum lights, used as stimuli, were controlled with respect to hue, saturation, and intensity. The threshold of the spectral light which was used to induce loss of sensitivity was determined at the end of each adaptation period. This was accomplished by the introduction of a motor rotated sector disc into the path of the stimulus light. The rotating disc could be removed at will. Determinations were made after varying amounts of time (from 2 to 300 seconds) and gathered together in the form of curves. Com-

parison could be made between different intensities of the same hue, but not between different hues since the thresholds for the different wave lengths have different values. Comparison is made, however, when the determinations are reduced to a percentage basis. The experiments were made with both a dark adapted and light adapted eye. Almack drew the following conclusions from her work:

1. For stimulus lights made equal in intensity, the sensitivity of the non-adapted eye is from greatest to least, yellow, green, blue, red. Initial loss in sensitivity is greater than at any other period. Initial sensitivity is greater for the dark-adapted eye than for the light-adapted eye, but both sensitivities tend to become equal with time. The rate of adaptation increases with the intensity of the stimulus.

2. For lights equalized for perceived saturation of the stimulus, the rates of adaptation to the various stimuli are very unequal. The largest rate (2, p. 115) "of adaptation by no means occurs at that intensity of light which gives maximum saturation to the color sensation. . . . The rate of adaptation is a function of the physical intensity of light, not of its subjective aspects, saturation and brightness."

3. For lights made equal with respect to wave length, the loss of sensitivity varies with the wave length. Under dark-adaptation, chromatic sensitivity is greater than for light-adaptation.

There is some confusion as to just what is measured by the technique described in this experiment. Consider a retina adapting to a stimulus light of a particular hue, saturation, and intensity. After a time, we determine the intensity limen of the eye to the same hue and saturation. This is true since we use the same source and introduce a rotating sectorized disc. If we consider the color solid to be a double cone, then in dealing with hues of maximal saturation which lie on the surface, the saturation of such hues is dependent on intensity. They are dependent variables. Then if the intensity of our stimulus light is reduced, we have also reduced the saturation. On the other hand, if the color solid is considered to be a cylinder (and this is implicit in the 'fundamental sensation curves' of König and Dieterici (22)) there is no relation between saturation and intensity. Under these conditions, measurements by the above sectorized disc can only indicate changes in perceived intensity.

Roaf (30), too, used the threshold of the eye as a measure of sensitivity. His apparatus was very complicated, but essentially it permitted the threshold to be determined after stimulation. He concluded that when any light stimulates the retina, the sensitivity, even of another part, is decreased to the short wave lengths and somewhat less to the long.

Kravkov (23) utilized the Helmholtz color mixer (18, p. 395 ff) in

1928 to test a theory of Lasareff concerning adaptation. The left field was fixated for ninety seconds and then matched with the right. The fixation was monocular. The match was made for intensity only. The results, claims Kravkov, support the outlined theory and indicate curves described by an equation with variable parameters. Kravkov's curves are, however, misleading. For example the first curve on page 93 (23) would fit the observation points just as well if it were completely inverted, or, indeed, if just a horizontal line were drawn. The conclusions to which Kravkov came are the following:

1. The decrease of intensity of a color sensation following color adaptation is of great importance and accords a lessening of the stimulus to 50% of its initial value.

2. The course of adaptation depends not only on the intensity of the stimulus but also on the hue. At the same intensity, violet (451 $m\mu$) is more quickly adapted than red (656 $m\mu$) and less than green (550 $m\mu$).

Helson and Judd (19) performed two important experiments in 1932. The first concerned itself with eye movements and their relation to color adaptation. An apparatus was constructed so that an individual placed his head in a sphere 36 inches in diameter. The inside of the sphere was lined with orange-red Hering paper. The subject fixated the inside of the sphere binocularly. Strong illumination eliminated pupillary changes. In theory the apparatus was so constructed that any eye movements did not change the effective stimulus. Observations were made for from five to seventy-five minutes. The conclusions arrived at by Helson and Judd by use of their 'adaptation sphere' are as follows:

1. The neutral condition of gray did not appear permanently. Gray was perceived but its perception was cyclic and spasmodic. They think that this was probably due to the pupillary effect.

2. The red component vanished making the perception more yellow.

3. Most of the adaptation took place within the first few minutes of stimulation.

In the second experiment, the subjects wore strongly selective red or green glasses (the transmissions are given in detail). The observer wore these glasses for as long as five hours and at hourly intervals identified colored Hering papers for hue, saturation, and intensity. It is difficult to understand how a subject wearing glasses transmitting nothing less than 600 $m\mu$ could see the blue component in a blue paper. Note this report (19, Table 2, p. 388). The results are as follows:

1. After five hours, the lighter colors appeared in the hue of the glasses while the darker colors were either neutral gray or complimentary in hue to that of the glasses (because of the after image).

2. At first there was a sharp loss in saturation as well as a shift in hue—a shift toward yellow.

Attention should be called to the theoretical explanations in this paper. Helson and Judd assume that if complete adaptation did occur while the individual is wearing colored glasses, he would see everything in 'natural' colors. This, they point out, is impossible if the glasses are highly selective since adaptation cannot supply what is physically not present. Further, they continue, if the glasses are not highly selective, the objects may appear in 'natural' colors since the predominant wave length will cause quick adaptation to itself and since the glasses transmit at least some of all the wave lengths, objects will be seen in 'natural' colors. This last is, of course, quite possible. Such non-selective glasses might lead to such a result. But adaptation with highly selective glasses can occur, at least theoretically. In this case everything transmitted would eventually be perceived as a neutral gray; perceived as a gray physical stimulus would be perceived by a non-adapted eye.

In 1934 Bouma (6) commented that when looking at a white surface with a black card at the center with sodium (or neon) light, the black part, after a time, took on a violet (or green) hue. His study, however, was more concerned with after images.

The first approach to the color adaptation problem by use of twin colorimeters was by Wright (47) in 1934. The apparatus was that described by him in (46). It can be characterized by the following. There were two eye pieces each of which was connected to a Wright colorimeter. Any hue could be seen in either of these eye pieces by adjusting three knobs associated with each colorimeter. The knobs mixed three spectral primaries. All hues in high intensities but not all saturations could be produced. The third eye piece, to the right of the other two, presented a field of white light. The subject was dark adapted for a period of thirty minutes. He then fixated the fields of the two colorimeters; the left field was seen by the left eye while the right field was seen by the right eye. Binocular rivalry did not occur, according to Wright. The test patch of the right eye was matched with the comparison field of the left. The calibration readings were noted by the experimenter. The subject then shifted his right eye to the eye piece showing white light and fixated this patch for three minutes. The left eye, during this time, was unilluminated. At the end of three minutes, the observer again fixated the colorimeter fields as before. The fields no longer match. The observer quickly matches the left patch with the perception of the right. When the match is completed, he signals the

experimenter who notes the time and the wedge readings, and the subject proceeds to make another match. The process is continued until no further change is noted.

This attack of the problem is somewhat different from anything else we have encountered. Here, it is the recovery time after adaptation that is measured. And recovery is given in terms of the spectral primaries of the colorimeter. At the beginning the two fields of the colorimeter are the same. The right eye is stimulated with white light which, it is assumed, depresses (or adapts) the activity of the receptor for the test patch to a level different from the original perception. The eye is then allowed to recover to the level of the test patch. This recovery is supposedly measured with the left eye. What is not taken into account, however, is that the eye does not recover to the level set by the test patch but continues to be stimulated by it until the receptors reach the equilibrium position. It appears that the white light is a confusing element and we wonder that Wright did not adopt the experimental procedure used in Cohen's (11) experiment. For additional papers using retinal recovery as a measure of adaptation see Allen (1) and the bibliography included in that paper.

Since the results of Wright's experiment are given in specialized response curves, no interpretation can be given until they are mathematically transformed to a more widely used system of primaries.

Color fatigue of the peripheral retina was investigated by Cogan and Cogan (10) in 1938-39. A perimeter was employed to hold the colors. The test color (Hering paper) was framed in a background having the same saturation as the test color. The time between the first perception of the color and its eventual merging into the background was considered to be a measure of adaptation. The results indicated the following:

1. Color adaptation continues to a neutral gray. The rate of adaptation increases toward the peripheral field.
2. Green was the most easily adapted color while red was the most difficult.
3. Dark adaptation influences color adaptation times.

There is a cursory study by Schouten and Ornstein (32) published in 1939. Their experiments utilize a separate field for each eye. They report that it was difficult to keep the two fields from fusing or overlapping. The comparison could be made in brightness only. Their contribution is that the adaptation of a test patch (here loss in intensity) is dependent upon the state of the rest of the retina.

Cohen (11) has recently completed an investigation in which he

utilized an instrument which presented two test patches to a binocular subject. The apparatus was so constructed that any hue could be placed in each patch by the adjustment of three controls. A test patch was placed in the right side of the instrument, a neutral gray on the left. After one minute the observer matched the left with what he saw on the right. The match being made, the left side is returned to gray while the right continues to adapt. After another minute, another match is obtained. In this way successive matches are made against time for a particular test patch. The instrument readings which are given in terms of monochromatic radiations of red, green, and blue (700 $m\mu$, 546 $m\mu$, 436 $m\mu$) are transformed to the monochromatic system of hue, saturation, and intensity. Thus *quantitative* changes in these latter variables were plotted against time. He did not employ intermediate colors.

Under the conditions of Cohen's experiment the following three laws may be stated:

Law I. The course of color adaptation consists of a gradual but never complete loss of saturation, with no change in hue, and an increase in intensity.

Law II. The rate and degree of desaturation depends upon the hue, the relative intensity, and the relative saturation of the stimulus color.

a. The loss of saturation for a green stimulus is most rapid, it is less for red, and least for a blue stimulus.

b. The less intense the stimulus, the greater the loss of saturation.

c. The less saturated the stimulus, the more rapid is the initial rate of desaturation; the equilibrium position, or point of lowest desaturation, is independent of the saturation of the stimulus.

Law III. The rise in intensity is dependent on the saturation and intensity of the stimulus and independent of the hue of the stimulus.

a. If the saturation and intensity of the stimulus are both high or both low, the rise in intensity will be greater than if one is high and the other is low.

SUMMARY OF METHODS

The methods used by the various investigators may be summarized as follows:

I. Introspective—mere description of adaptation.

De La Hire (12), Aubert (3), Ssamajlow (34), Sheppard (33), Troland (41), Helson and Judd (19), Bouma (6)

II. *Controlled introspective—perceived objects are carefully controlled (no comparison with unadapted portion of retina).*

Bokowa (5), Burch (8, 9), Beck (4), Edridge-Green (13), Sheppard (33), Helson and Judd (19)

III. *Liminal-limen (intensity) is measured after adaptation.*

Almack (2), Roaf (30)

IV. *Comparative monocular introspective—two fields viewed monocularly—differences are noted introspectively.*

Exner (14), Porter and Edridge-Green (29)

V. *Comparative monocular non-introspective—two fields viewed monocularly—match made on instrument.*

Exner (14), Schön (31), Hess (20, 21), Voeste (45), Edridge-Green (13), Kravkov (23)

VI. *Comparative binocular introspective—two fields viewed binocularly—differences noted introspectively.*

Brückner (7)

VII. *Comparative binocular non-introspective—two fields viewed binocularly, one field for each eye—match made on instrument.*

Wright (47), Hamilton and Laurens (17), Schouten and Ornstein (32)

VIII. *Comparative duo-binocular non-introspective—two fields viewed binocularly, both fields seen by both eyes—match made on instrument.*

Cohen (11)

SUMMARY OF RESULTS

The results obtained by the various investigators may be summarized as follows:

I. Saturation

Partial loss: Bokowa? (5), Aubert (3), Exner (14), Troland (41), Helson and Judd (19), Almack? (2), Roaf? (30), Cohen (11)

Complete: Edridge-Green (13), Sheppard (33), Cogan and Cogan (10)

II. Intensity

No change: Burch (8, 9), Sheppard (33)

Loss: Schön (31), Voeste (45), Almack (2) Hamilton and Laurens (17), Roaf (30) Kravkov (23), Schouten and Ornstein (32)

Increase: Cohen (11)

III. Hue Shift

Investigator	Basic Colors*				
	R	Y	G	B	V
Bokowa (5)		x		x	
Exner (14)	656-589		527-517		431
Hess (20, 21)	650	565		490	
Voeste (45)		560		498-494	470-460
Sheppard (33)		x		x	
Troland (41)	x	x	x	x	
Helson and Judd (19)	.	x			
Cohen (11)	no shift				.

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THE EFFECTS OF NOISE*

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The development of modern industrial society with its concentration of population and machinery in confined areas has brought with it a concomitant concentration of noise. The popular science literature of the past twenty years is dotted with articles describing the deleterious effects of noise in highly emotional terms unwarranted by the facts. Few problems have attracted a wider range of professional interests than the problem of noise and what to do about it. Physicians, public health authorities, architects, psychologists, otologists, physicists, sound and electrical engineers have contributed to the literature. On the industrial side interest and concern is evidenced by the telephone and radio industries, automotive, airplane and railroad equipment designers, manufacturers of office equipment, building materials and a wide range of consumer appliances. Most recently the safety engineers and insurance companies have expressed an interest because of the growing recognition of occupational deafness. One is reminded also of the various noise abatement campaigns which sprang into prominence in the early 1930's.

In the light of this wide spread interest, one might reasonably expect to find a considerable body of well grounded scientific literature relevant to the topic of noise and how it affects the behavior of man. On the contrary there is a paucity of studies dealing with the problem at the human level in terms which provide a basis for generalizations even though a wealth of unsystematized experience by acoustical engineers points to the conclusion that people are benefited by noise reduction. Part of the reason for this state of affairs lies in the fact that only within the past 20 years has there been any attempt to standardize terminology and techniques in the measurement of noise itself. Even today the available procedures are still in the developmental stage where many improvements are to be desired.

It is the purpose of this review to summarize critically those studies pertaining to the effects of noise on human beings. Occasionally it may be necessary to refer to studies of animals to point up certain areas but in general the emphasis will be upon studies using human subjects.

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Consideration will be given in turn to the effects of noise on output speed and accuracy, fatigue and energy expenditure, the course of noise-adjustment, stimulation deafness, and finally the factors determining relative annoyance. Preliminary to an examination of these problems it is necessary to summarize the methods of measuring noise and their limitations.

DEFINITION OF, AND PROCEDURES IN MEASURING NOISE

Noise has been defined as unwanted sound (60). This statement bearing as it does a heavy value judgment appears to be widely accepted among physicists, engineers and the like. Paradoxically the psychologists lean toward a definition couched in more physical terms. Thus Dockeray (11) writes, "If a complex of vibrations is not in a harmonic ratio, we speak of the result as noise." And Dashiell (7) agrees in saying, "When air vibrations are non-periodic and irregular, or are less than two full vibrations, they produce *noises* rather than *tones*." The present paper is primarily concerned with noise as unwanted sound recognizing at the same time that the degree of annoyance may vary from a theoretical zero to some maximum point depending upon factors to be enumerated later.

The unit of noise measurement most widely used is the decibel. This is a logarithmic unit based on sound energy measurements. Two sounds differ by N decibels when $N = 10 \log_{10} E_1/E_2$. (E_1 = sound energy density of sound 1; E_2 = sound energy density of sound 2.) In practice it has been found that 1 decibel represents *approximately* 1 J.N.D. in a 1000 cycle tone. The zero point on the decibel scale could be set at any value but has now been standardized at 10^{-16} watts per square centimeter which is a convenient value somewhat lower than the normal threshold of hearing for a 1000 cycle tone. Publications prior to the tentative adoption of standard accoustical terminology in 1936 were often confusing in their use of the decibel scale because the minimal auditory threshold varies with frequency of vibration. Early writers refer to a tone of 200 cycles at 30 db without specifying whether they mean 30 db above the 1000 cycle tone threshold or 30 db above the 200 cycle threshold.

Less frequently used in this country is the word *phon* which is numerically equivalent to a decibel when considering the standard 1000 cycle tone. For any tone other than a 1000 cycles its phon value is determined by equating in loudness that tone with the standard. Referring to Figure 1 this means that a 300 cycle tone having an intensity level of 40 db. above 10^{-16} watts per square centimeter will appear to

be equal in loudness to a 1000 cycle tone having a 30 db. intensity level above the same base. Hence the 300 cycle tone at that intensity has a loudness level of 30 phons.

Related to this unit are the terms *loudness units* and *sones*. If a 1000 cycle tone at 40 db. be taken as a standard and a second tone be reduced to one-half the subjective loudness of the standard, the second is said to

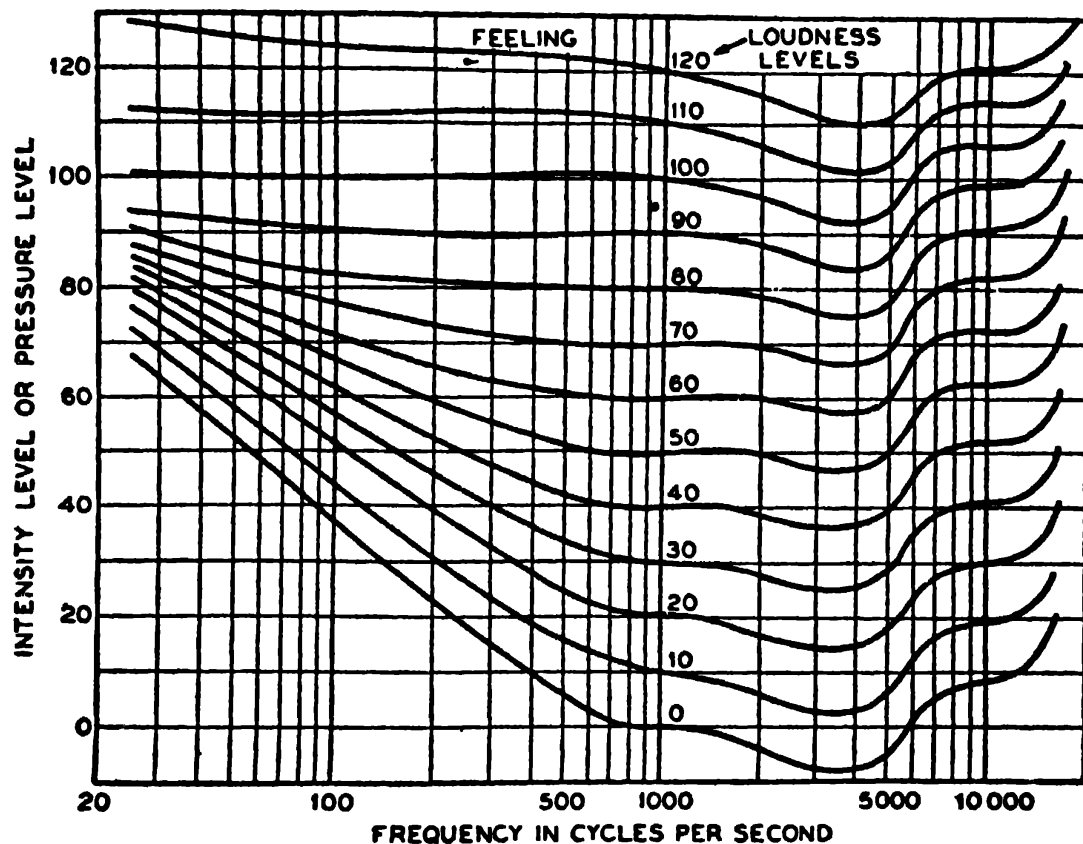


FIGURE 1. EQUAL LOUDNESS CONTOURS (30)

be one-half a sone. Conversely a tone adjusted so that the normal observer judges it to be twice as loud as the standard is said to be 2 sones.

Stevens proposed this latter unit and has published curves (51) showing the relation of sones to db. level for a number of representative frequencies. The sone curve for tones between 700 and 4000 cycles is almost identical with a curve of loudness units (L.U.) published by Fletcher (16) and subsequently adopted with slight modification by the American Standards Association (60). The L.U. curve was constructed on the basis of several independent studies conducted in this country and in England where estimates of various fractional and multiple loudnesses were plotted against phons. The chief difference between the L.U. scale and the sone scale is in the magnitude of the numerical

values assigned, the L.U.'s being roughly 1000 times greater than sones. Either scale is more indicative of the subjective effects of noise level changes than the db., phon or the direct intensity scales. Although the American Standards Association has specifically defined decibels, phons, and loudness units, it is common practice at least in speaking and often in writing to use the term decibel to cover physical sound intensity, equal loudness contours, or subjective loudness. It would be more meaningful to persons unfamiliar with acoustical terminology if phons were translated into either sones or loudness units.

One practical way by which proper usage of these terms could be encouraged is by redrawing the equal loudness contours of Figure 1 (a figure that appears in almost every acoustical discussion) in terms of either sones or L.U.'s. As the figure stands the spaces between the contours do not represent equal increments in loudness. Stevens and Davis (51) have already made a step in this direction by assigning equivalent sone values to the contours as already drawn. The translation into L.U.'s can be made by using Table I.

TABLE I
EQUIVALENT PHON AND L.U. VALUES

<i>Phon</i>	<i>L.U.</i>	<i>Phon</i>	<i>L.U.</i>
110	215000	50	2200
100	88000	40	980
90	38000	30	360
80	17100	20	100
70	7950	10	10
60	4350		

Figure 2 is an attempt to make the equal loudness contours more meaningful by translating phons into L.U.'s ($\times 10$) using the table published by the American Standards Association (60) and then plotting the contours so that each successive contour going up the scale represents a doubling of the loudness.

Two basic methods have been employed in measuring the loudness of sounds. The *first method* or older of the two is the binaural matching method with or without an off-set receiver. In this procedure a normal observer is provided with a receiver being driven through a calibrated attenuator by some kind of oscillating circuit. While listening to the sound to be measured with one ear he matches the loudness in the other ear by adjusting the attenuator. The attenuator has usually been cali-

brated in terms of db. above the threshold for a 1000 cycle tone. A variation of this method employed in some of the earlier work was the use of a phonograph record of a warbling tone. The gain control was calibrated in decibels above the hearing threshold.

Some question has been raised regarding the advisability of using a receiver, offset from the ear a centimeter or more, for the purpose of mixing the known and unknown sounds. Those favoring the use of an offset argue that comparisons are made more accurate because both the

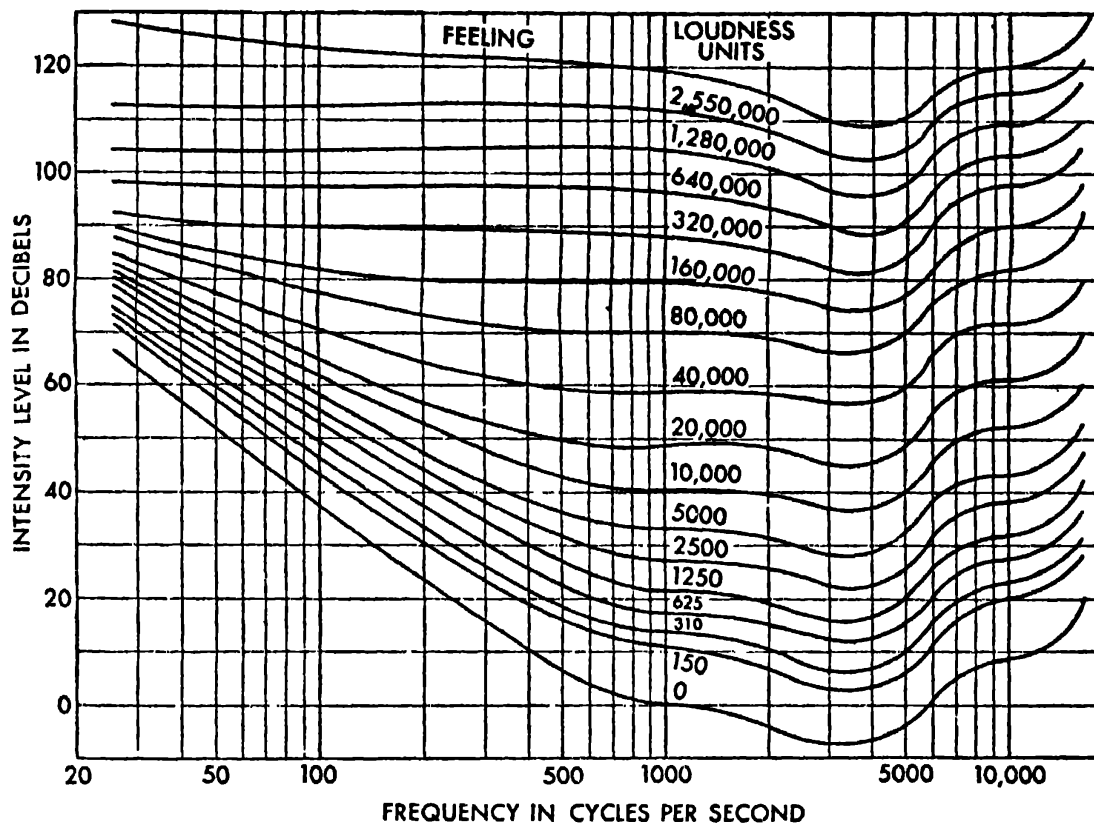


FIGURE 2*. EQUAL LOUDNESS CONTOURS EXPRESSED IN LOUDNESS UNITS ($\times 10$)

known and unknown sounds are matched in a single ear. On the other hand others (5) insist the offset has a baffle effect on the incoming waves from the unknown source distorting their loudness. Practiced observers following a rigid variant of the method of limits familiar to psychophysicists have in this manner measured complex sounds with a fair degree of reliability. "The spread of the mean final settings normally does not exceed ± 2 db. at 100 db. level, ± 4 db. at 80 db., and ± 6 db. at 50 db." (5). Some experience using a receiver without an offset while

* In preparing Figure 2, loudness units were multiplied by 10. To reduce, neglect the final zero, i.e. 2,550,000 = 255,000 L.U.'s and so on.

matching the loudness of an unknown noise indicates that the position of the receiver is highly critical. Slight pressure on it or a small shift with respect to the meatus produces large changes in the apparent loudness of the calibrated sound.

Using a different arrangement Geiger and Abbott (20) presented twenty observers with sounds from a variety of sources, (buzzer, rattler, vacuum cleaner, bell, etc.) by means of a microphone, amplifier, and loud speaker which were to be matched in loudness by a 1000 cycle tone introduced also through a loud speaker. The mean difference for all sounds and all observers between the sound level as measured by a meter and the matched tone was 2.4 db., the meter reading being between 76 and 40 db. Generally the observers matched the unknown sound level with a 1000 cycle tone at a higher energy value than the sound level meter indicated. It is significant that Geiger and Abbott concluded, "This was a very severe test indeed, and the meter measurements were completely confirmed."

The *second method* employed in measuring sound levels is the objective method in which a microphone is placed in the sound field and the electrical output of the microphone is amplified. An adjustable weighting network may be interposed between this amplified current and the indicating meter usually calibrated in decibels. The meter actually indicates A.C. power which is proportional to the rate of reception of sound energy at the microphone. The weighting network is adjustable so that the response of the instrument will approximate the response of the ear to the sound energies falling on it.

Referring to Figure 1, it can be seen that between 30 and 1500 cycles at 100 db. energy intensity, loudness remains practically constant. However, if the energy intensity level is reduced to 40 db. the frequencies below 90 cycles would not be heard and those above 90 would vary from just perceptible to 40 phons in loudness. Consequently a noise meter must be adjustable so that at high intensities it gives a reasonably constant output for all frequencies and with low intensities it should give little or no response at low frequencies, but should increase its output as the frequency is raised to approximately 2000 cycles and then decrease as the frequency exceeds this level. Since it is not at present practically feasible to build a portable meter with networks permitting response curves similar to each of the loudness contours most commercial sound-level meters are provided with three such networks adaptable to levels approximating 40, 70, and 100 db. in over all intensity.

Under special conditions where the noise to be measured is made up

of a number of harmonic frequencies as is the case with the noise from large transformers the objective method gives readings that may be as much as 30 db. *lower* than the aural comparison method (5). The reason for this lies in two related facts. First, frequencies close to, but not exactly equal to the harmonics of a given fundamental exert a masking effect on that fundamental thereby reducing the loudness which would otherwise be present. Second, when a number of harmonic components are present in the noise being measured their energies add to, rather than subtract from the loudness in a manner not provided for in the sound-level meters now available (56). The meters commercially available compensate for the masking effect of the near-harmonics but do not sum the sound energies of the harmonics as the ear does. Geiger and Abbott noted in the report referred to above (20) that in cases where the sound was composed of a large number of "component musical notes, well separated in frequency and of nearly the same loudness" the individual ratings of loudness were considerably higher than the meter readings.

Another limitation of sound level meters traceable to the microphone design is the wide tolerance permitted in their response characteristics by the American Standards Association. These are such that a 90 db. reading may have an error of plus or minus 3 db. On the surface such an error may appear small yet if one should take a low reading instrument to measure the sound level in a given area of approximately 90 db., then return later after acoustical treatment with a high reading instrument, no change would be registered. Subjectively, however, unpracticed observers might judge the noise after acoustical treatment to be half as loud as before. Indeed, a change of 6 db. at that level is almost equal to doubling or halving the loudness as determined by a number of independent investigators using trained observers under careful laboratory conditions (19, 24, 26). In many field measurements of noise these errors are small relative to the fluctuation in sound energy from moment to moment and place to place.

It becomes clear that the aural comparison method is superior to the objective method in some instances provided that competent observers are employed, and the test tone is properly calibrated. The known variation in individual observers are of the same order as the allowable tolerances in microphone response characteristics. In addition, however, the sound-level meters even if accurate in their response to the sound energy of the field do not register the non-masking effects of harmonic components in a sound spectrum. Undoubtedly one of the chief reasons for the wide use of sound level meters rather than the aural

comparison method lies in the fact that the meter readings are repeatable with a high degree of reliability. Furthermore, it is difficult if not impossible for an observer to match sounds varying greatly in intensity from moment to moment. The aural comparison method does show differences in loudness when the same observer makes repeated observations of the same sound. This is especially true of inexperienced observers and leads investigators after a brief trial of the method to discard it. On the other hand the work of Geiger and Firestone (21), Ham and Parkinson (23), and Fletcher and Munson (17) showed that practiced observers following carefully a standard procedure are remarkably reliable in their estimations of loudness of relatively steady tones.

It is possible to compute the loudness of a complex noise by the use of a wave analyzer which records the magnitude of sound energy in each of several wave-frequency bands. The noise spectrum thus determined provides the basic data for computing the loudness using formulae developed by Fletcher and Munson (17). The method is cumbersome and yields results only slightly different than the mean value of several observers using a properly calibrated test sound.

It becomes clear that both methods of measuring noise levels requires more than an ability to set up a microphone, flick switches and read dials. The meter readings must be interpreted in the light of the nature of the sound spectrum and the known tolerances of the meter itself. On the other hand the aural comparisons must likewise be conducted and interpreted by one familiar with the standard manner of listening and the known sources and magnitude of errors. In the hands of competent investigators either method will not give a measure of high precision but will provide a basis for an informed estimate of loudness.

EFFECTS ON PRODUCTION

One may find in textbooks references to a number of reports indicating that production is markedly increased with a reduction in noise. Several of these emanate from an anonymous article (62) in which references to the original reports are lacking. It is not possible, therefore, to evaluate the claims that "lowering the noise level in a telephone-exchange room from 50 db. to 35 db. resulted in a forty-two per cent reduction in errors and a three per cent reduction in cost per message" (4). Or that "moving the assembly department of a temperature-regulator company from next to the boiler room to a quieter location reduced faulty assemblies from seventy-five per cent to seven per cent of those turned in for inspection" (4).

Only one field study in this country has been completed with sufficient control and covering a sufficient time to place confidence in the results. This study has not been formally published in detail although it has been referred to frequently (32, 59). The Aetna Life Insurance Company in December 1928 installed sound absorbing materials in an office occupied by typists, clerical checkers, punchcard and comptometer operators. Bonus figures were available reflecting the efficiency of employees for a year prior to installation. This "efficiency" refers to the allowed time for a given amount of work as determined by time studies, divided by the actual time taken. The average difference in the bonus based on efficiency of fifteen clerks taken at semi-monthly intervals for the year prior to, and a year after acoustical treatment was 9.2% in favor of the quieter condition. At no time during the "quiet" year did the bonus go below the level of the first year. No other changes were introduced into the working conditions during these two years. The sound level as measured by a Western Electric 3-A audiometer using the loudness matching method was 35 "sensation units"* after acoustical treatment.

A control check on these findings was made after one year by covering the sound absorbing materials with gypsum board. The sound level was then 41 "sensation units." The bonus efficiency immediately dropped to an intermediate value between the two previous years, but within two months was approximately equal to the level of the "quiet" year. At the time the bonus reached this high level in the presence of the greater noise there was a reduction in work, only the more efficient employees being retained from then on, which probably accounted for the high average efficiency. Comparable, but less clear-cut results were noted among the punchcard, and comptometer operators. In these latter groups machine breakdowns and other uncontrolled factors complicated an interpretation of the data.

The available data provides no basis for the calculation of statistical tests of reliability of the differences noted. In spite of this fact it is likely that the advantage noted in favor of the quieter condition is not a matter of chance. •

The influence of noise in school children and grade school work was investigated by Obata (39). The noises were a variety of phonograph records supplemented by a mechanical rattler varying between 60 and 85 db. With each type of sound working speed was more affected than accuracy in arithmetic and symbol cancellation. The adverse effects

* This is a unit roughly equal to a decibel and used prior to 1936.

were not solely related to intensity since certain soft records were as distracting as the rattler.

At the Waverly Press, Baltimore, a number of changes were made in working conditions in 1935, among them being an acoustical installation. It was reported that, "The reduction in noise and the use of rest periods are found to be useful means of producing not only higher efficiency from the standpoint of management but also a much more agreeable situation for workers themselves" (48).

In an attempt to duplicate the industrial production situation Laird (29) devised an "experimental factory" in which the subjects were required to touch a metal plate with an electrical stylus through 10,500 small holes per work spell as they appeared in a moving tape. The holes were irregularly spaced and of irregular size appearing in a window $3\frac{1}{2}$ by $1\frac{1}{2}$ inches directly in front of the worker. Production of four subjects, throughout each of the $4\frac{1}{2}$ hours work spells was recorded by electrical counters. The results presented in the form of the number of holes missed per working spell per worker showed almost a straight line drop from 400 holes missed at 90 db. to 300 missed at 40 db. when the complex noise delivered through an amplifier by the Western Electric 3-A audiometer was presented. Using pure tones at loudness levels equal to a 512 cycle tone at 60 db. on the Western Electric 2-A audiometer, errors increased slightly as the frequency was stepped up from 64 to 512 cycles. Beyond this frequency, errors increased more rapidly from 270 at 512 cycles to 325 at 4096 cycles. It is to be noted that the complex noise especially rich in high frequencies at comparable loudness levels affected production more adversely than the pure tones.

In evaluating this study one wonders to what degree the increase in errors both with increases in frequency and loudness might be due to progressive dulling of motivation. In spite of efforts to keep motivation constant the high error counts occurred after several days work with high level and high frequencies. On the other hand, one might have expected a gradual adaptation to the noise such as Harmon (24) found, in which case errors would have been least numerous toward the end. It is regrettable, though understandable in view of the lengthy demands on the subjects, that the study was not repeated using the noise intensities and frequencies in reverse order.

Pollock and Bartlett (44) conducted a series of experiments using loud clicks and a complex noise delivered to head phones worn by the subjects while engaged in a simple eye-hand coordination task of picking up and replacing pegs in a moving peg board apparatus. The results indicated that the noise adversely affected performance initially but

adaptation set in rather quickly especially when the task was of such a nature as to permit thorough automatization. Considering long work spells of eight hours the difference in performance between noise and quiet was insignificant. In the case of mental work (word-making) noise, especially discontinuous complex tones, had an adverse effect on speed which was most marked early and late in a working spell of thirty minutes. Again, however, a plan of work, rendering performance more automatic tended to counteract adverse effects. No data are presented of the loudness or frequency of noises used, but performances appeared most affected by the arrhythmical character and interestingness of the distractions.

Weston and Adams (57), in England examined the output of weavers over a period of twenty-six weeks during which the workers wore ear defenders on alternate weeks, thus reducing noise from 96 db. to 87 db. as measured by the Barkhausen audiometer (comparable to the Western Electric 3-A, although the reference level for the db. scale is not available) output was one per cent greater while wearing the ear defenders. However, output was largely controlled by loom speeds. The operations by the weavers occupied about five minutes per hour without ear defenders, but with ear defenders were reduced to four and one-half minutes, a speed increase of about twelve per cent. There was some evidence based on hourly records that even after years of work in a noisy environment, the worker does not become completely adapted to noise but goes through the adaptation process daily.

Since the weather in the above study favored work on weeks when ear defenders were worn the same investigators returned to the same weaving shed to repeat their observations over a twelve-month period using two groups of 10 weavers each equated by supervisor's ratings. They discovered a three and one-half per cent advantage in output and a thirteen and one-half per cent improvement in personal efficiency in the group wearing ear defenders which reduced the noise from 96 to 81 db.

The significance of these experiments lies in the apparent demonstration that workers long accustomed to noise, and by no means disposed to complain of it, may show substantial increases in working efficiency as a result of noise reduction. Such a finding is obviously of economic importance in the manufacturing field. The investigation, therefore, merits careful critical consideration.

The second experiment is open to the serious objection that the exact equality in production of the experimental and control groups was never demonstrated. The unreliability of the rating method might have pro-

duced a chance superiority for the experimental groups. It is regrettable that the investigators did not interchange the experimental and control groups.

The influence of suggestion was not controlled. How important a factor suggestion may be can only be surmised. Baker (3) demonstrated with college students working for 5 minute periods under noise and quiet conditions that output can be lowered or raised depending upon subtle suggestions incorporated in the pre-experimental instructions. We have no way of knowing what suggestions were given workers in the Weston and Adams studies. However, since neither the work, nor the length of the work spells in Baker's study are comparable to the industrial situation there is no reason to expect suggestion would be a major factor influencing personal efficiency.

Finally, the variability of percentage improvement in personal efficiency from month to month ranging from 31.42% to 3.42% suggests powerful uncontrolled factors influencing the differences between the groups.

There remains, none the less, the fact of a consistent superiority of output under sound reduction. The authors' conclusions that "excessive noise is a factor having an important effect upon industrial efficiency" and that "tolerance is not acquired with lapse of time" are not irrefutably established, but the weight of evidence is in their favor.

A study by Kornhauser (28) attempted to relate the output of typists to noise in a regular office situation. The results were equivocal and complicated by the existence of other distractors in the quieter location.

Using only two subjects, presumably the authors, Vernon and Warner (54), reported that performance on an arithmetic test, a purely routine assembly job, and reading difficult material were not significantly affected by noise after a short adjustment period during which there were adverse effects. On the other hand judgments of the magnitude of subjective disturbance indicated less rapid adjustment to the noise. These conclusions were based not only upon observations under laboratory conditions but included tests made in the presence of factory and office noises.

In reviewing these reports dealing with production and accuracy of work one is impressed with the methodological difficulties of conducting meaningful investigations "in the field." Other conditions beyond experimental control, such as temperature, machine breakdowns, or changes in quality of raw materials may obscure whatever benefits noise reduction may contribute. It seems clear that a thorough study of industrial pro-

duction as contrasted with office work has yet to be done under positive control of all factors, covering work periods of sufficient length with noise typical of industrial situations. Furthermore, the factor of suggestion in the studies so far published has received little attention possibly because it is difficult to control.

INFLUENCES ON VITAL PROCESSES

The effect of noise on blood pressure, respiration rate, metabolism, muscular tension, electrical skin resistance, digestive processes and similar functions has attracted some experimental investigation. Among the most widely quoted of these is the report by Laird (29) who measured the metabolic rate of four typists during a half hour of resting and while typing a standard letter over and over again for two hours per day throughout a four-week period. During the first and last of these weeks the walls of the testroom were bare. During the second and third weeks sound absorbing material was applied to walls and ceiling reducing the noise produced mechanically from 50 "sensation units" to 40 "sensation units."

On the average the metabolic rate was fifty-one per cent higher while working compared with the resting rate during the quieter weeks, and was seventy-one per cent higher during the noisy weeks. A suggestion that the noisy phase was more fatiguing is found in the data showing the average time for the last five letters of the two-hour spell was seven seconds less than for the first five letters in the quiet phase, while the comparable time was five seconds more in the noisier condition. However, the fast typists improved in speed when the noise was reduced, while the slow typists showed little or no change in overall speed.

No day-to-day data were published by Laird, a fact that has been criticized by Harmon (24), who found a very rapid adjustment to noise, using metabolic rate as the principal indicating function and mental arithmetic as the work. Such a task has a much lower muscular component than typing and could not be expected to affect the metabolic rate to a marked degree. It is important to note that on the initial days of noise Harmon found a thirty-seven per cent increase in metabolic rate. During the same days the average number of errors per day was 12 for quiet and 24.3 when working under noise.

Prior to Laird's study Morgan (38) found evidence of similar extra effort in the presence of noise. His subjects engaged in a form of translating letters into numbers by pressing appropriate keys. Respiration was recorded as well as the pressure exerted upon the keys. The noise came from a variety of buzzers, bells and phonograph records. Noise and

quiet periods alternated during the experimental sessions; the ten minute noise period following thirty minutes of quiet. Noise was responsible for greater key pressure being exerted and greater articulation on the part of the subjects—a device spontaneously adopted in an apparent attempt to overcome the distracting effects of the noise. The time per response increased significantly at the beginning of each noise period but subsequently decreased to a point where output was more rapid than during quiet.

Vernon and Warner (54), Freeman (19), and Ford (18) generally confirm the findings of Morgan and Laird so far as an increase in energy expenditure in the presence of noise is concerned.

Davis (9) measured muscle tension in the presence of noise by means of action potentials. His eighteen subjects were required to do nothing except to sit and listen to the noise presented in a series of two-minute intervals separated by two minutes of quiet. Marked increases in tension appeared on the first experimental day at the onset of each noise period. On subsequent days the change became less and less marked so that by the fifth day tension was virtually unchanged when noise was presented.

The effect of noise on digestive processes was examined by Smith and Laird (49) whose four subjects swallowed a rubber balloon which was later inflated in the stomach. Peristaltic contractions were thus recorded by an appropriate pneumatic system operating a writing point on a kymograph. Noise of 80 db. for ten minutes following a twenty minute quiet period produced a thirty-seven per cent decrease in the number of contractions. The recovery period following the noise showed no consistent pattern or change in stomach action. The effects using a 60 db. noise were less marked but of the same nature. The rate at which saliva and gastric juices flowed was also decreased in the presence of noise. These changes would presumably have an adverse effect upon the total digestive process.

Although details of the experiment have not been published in full, Luckiesh (33) reported that a "demonstration visual test" was performed six per cent more quickly in a quiet room than in the presence of the hum from a motor-generator set.

Other observations in the presence of noise of high intensities and short duration have demonstrated increased blood pressure (35) and increased cranial pressure (26). In the later case, however, one wonders to what degree the sound pressure itself may have affected the recording apparatus.

Davis (10) reported a marked decrease in skin-resistance at the onset

of a five-minute noise period. However, the function gradually returned to normal and reached a value before the end of the noise period approximating the value obtained with a control group sitting quietly for ten minutes. The noise level is not reported.

Even though no experimental studies have been reported dealing with effect of noise on emotional control or emotional stability, the reports above indicate physiological changes of the kind associated with emotional disturbances. Many of the changes noted in response to noise are similar to, if not identical with those produced by stimuli exciting fear, anger, and general emotional tension. Throughout much of the published literature (15) are testimonials that cannot be dismissed lightly indicating that high noise levels are the cause of "nervous exhaustion" emotional instability and related conditions.

ADAPTATION TO NOISE

In some of the studies previously cited, notably those by Harmon (24), Morgan (38), Ford (18), Davis (10), Pollock and Bartlett (44), emphasis has been given to the process of adaptation or adjustment to noise. Morgan found an initial decrement in output followed in some instances by greater-than-normal production. Harmon reported an initial increase in metabolic rate followed by smaller and smaller increments on succeeding days at the onset of noise. Pollock and Bartlett's subjects reported they could hardly bear the noise in the early experimental sessions yet at the end they did not notice the noise. These reports do not fit with the long term studies by Laird, Aetna Life Insurance Company, Weston and Adams, which show a net deleterious effect of noise. It is possible that the differences in results can be attributed, in part, to the factor of suggestion as already mentioned in the previous section. On the other hand it is not reasonable to expect that a higher-than-average efficiency index could be maintained without a break over a full year of operation in a group of fifteen clerks by suggestion alone, as the report of the Aetna study shows. It is also true that the studies showing an adjustment to noise such that the detrimental effects are reduced or eliminated cover, for the most part, short noise periods. We may hazard the guess that if the noise were continued over longer periods comparable to those experienced in industry the adjustment mechanism compensating quickly for the initial ill effects may break down and the net effect of noise even in later studies might have been adverse. In line with this hypothesis is Freeman's (19) idea that the added effort noted in the presence of the initial noise is not just a rise in total energy expenditure but comes from a shift in the

pattern of supporting processes, not directly concerned with the principal functions (typing movement, etc.) under observation. With continued exposure to the noise this irradiation of excess tension becomes less prominent and a more economical concentration of supporting processes reasserts itself.

FACTORS AFFECTING THE ANNOYANCE OF NOISE

Throughout the studies mentioned above are frequent references to the erratic manner in which noise acts as a distractor. Only a few studies have been reported which were aimed at discovering the features of the noise which contributed to annoyance. Using a paired-comparison method with pure tones at 50 db., Laird and Coye (31) found the annoyance of tones to be ranked in the following descending order: 8192, 4096, 64, 2048; with 128, 256, and 1024 of minimum and equal annoyance. Thus, other things being equal, high tones and extremely low tones are judged more annoying than those in the middle range. The effect of intensity was examined by matching in annoyance a 256 cycle tone at various intensities with other pure tones adjustable in loudness. The equal annoyance contours thus established below 500 cycles followed roughly the equal loudness contours shown in Figure 1. Above that frequency the equal annoyance contours tended to drop away from the equal loudness contours, indicating that low tones of high intensity are as annoying as high tones of relatively low intensity.

The observations of acoustical engineers (40) testify to the importance of high frequencies as they affect the reported comfort of treated areas. In one case two adjacent offices were acoustically treated, but with different materials both having the same noise level and similar sound sources. The absorption coefficient at 4096 cycles for Office A was 0.45 and for Office B was 0.82. B was rated by its occupants as comfortable while those working in A complained that the typewriter noise was not properly quieted. Replacing the lower coefficient material by the other type corrected the complaint, in spite of the fact that the overall Noise Reduction Coefficients for the two materials were approximately the same.

Interrupted noise or discontinuous tones have been generally found to be more annoying than steady noises. This was true in the reports by Pollack and Bartlett (44), and Laird (24).

Furthermore Finch and Culler (13) found greater auditory destruction in dogs subjected to high intensities when the tones were interrupted. Even the reports of Morgan (38), Davis (10), Freeman (19),

Harmon (24), and others who have used relatively short noise exposure periods would lead one to expect that discontinuous noise would be the more detrimental. In all of these reports the initial onset of noise produced the greatest adverse effect. Hence the discontinuous noise would be somewhat comparable to a rapid series of noise periods each of which demands a new adjustment with initial phases of maximum cost to the individual.

A number of writers have referred to a miscellaneous list of factors that influence the annoyance of noise, none of which have been subjected to systematic investigation. Among these factors are the unexpectedness of noise (42), the spreading effect and reverberation (47) the degree to which the noise is unnecessary or indicates malfunctioning of equipment (2).

Of primary interest to psychologists are the individual differences in noise tolerance. Numerous independent observers in the field (47) as well as nearly all laboratory investigations agree in reporting wide differences in the degree to which persons are affected by the same noises. Related to this factor is the wide variation in response by a given person on different occasions. Shifts of attitude, motivation, and attention result in widely different types of performance under the same external noise conditions. No systematic attack has been made on this problem to discover what personality feature or other psychological factors are correlated with noise tolerance.

STIMULATION DEAFNESS

Kemp (25) has recently reviewed the studies dealing with stimulation deafness. It is, therefore, unnecessary to review that area except to reemphasize the necessity of interpreting the reported incidence of alleged occupational deafness in the light of the normal decrement in acuity with age. The most complete information dealing with incidence of hearing losses in the general population is that gathered by the Bell Telephone Laboratories at the New York and San Francisco Fairs in 1939 (50). In spite of this precaution which prevents the acceptance of many reports at their face value, there is little doubt that extended exposure to intense noise will produce both a temporary and permanent loss in auditory acuity.

Much of the experimental work in this area has been done on animals in an effort to throw light on the mechanism of hearing. The results have shown in general that the frequencies suffering the greatest impairment are not correlated with the predominant exposure fre-

quencies, although the degree of impairment is related to the duration and intensity of the exposure noises (8, 43, 46, 55).

Although numerous reports are available purporting to show that workers in high noise levels have a greater-than-average incidence of hearing loss (22, 34, 35, 36, 37, 40, 45, 52, 53), the devices for measuring hearing have varied widely making any comparison between occupations hazardous. One survey (61) has been published of nine plants in New York State conducted in a standard manner using a Western Electric 3-A audiometer. The report concludes that the greatest percentage of deaf people occurs in plants of greatest noise. However, it must be pointed out that not all employees in the plants of greatest noise were tested. It is possible that a selective factor operated to exaggerate the proportion of deaf people tested in those plants.

There appears to be no reasonable doubt that high noise levels can and do produce hearing defects especially after long exposure. Still unanswered are the questions, what levels can be tolerated, and for how long without incurring a defect?

CONCLUSION

In spite of a wide spread interest in noise abatement relatively few facts have been well established. Popular literature not covered in this review abounds in emotional outbursts against the baneful effects of noise. Public support has been enlisted for noise abatement campaigns on the uncritical acceptance of the assumption that noise because it is annoying must be harmful. The available scientifically controlled studies are not in complete agreement but tend to show ill effects in output, speed of work or vital processes. Although a considerable degree of adaptation takes place, the evidence suggests that it is seldom complete. Marked individual differences in susceptibility to the ill effects of noise have been noted but no reported attempts have been made to correlate these differences with other facets of personality. The factors determining annoyance have not been subjected to thorough analysis. Stimulation deafness is an unquestioned result of exposure to loud noises for long periods but its extent and the critical levels of noise necessary to produce it in humans have not been clearly established. In summary, it is clear that there are many circumstances under which noise detracts from efficiency and well-being. Under what circumstances noise is deleterious and for what kinds of people are questions for further fruitful research.

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STUDIES IN TIME PERCEPTION

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The study of time takes various forms and presents problems to investigators in many fields. For example, the nature of time is of special interest to the philosopher. The accurate measurement of time is important for the physicist and the astronomer. In a somewhat related sense, time is a very practical problem for the daily worker who punches a time clock, the athlete who competes in games and the aviator in his maneuvers. The perception of time is a subject of special interest to the psychologist.

In this paper we shall be interested in time perception and time estimation and shall be concerned with such problems as the nature of time only in so far as it is necessary for a definition of our problem. In this quest we will not be greatly aided by such definitions of time as that proposed by Kant who said that time is "a universal form of intellect" or by Leibnitz who described it as "the obscure and confused picture of the grounds which determine the order of succession."

The physicist's definition of time, as the measurement of the movement of the earth through space, is more helpful but it is not entirely satisfactory for our purposes. Neither is the somewhat naive operational definition of the psychologist who says that time is what the clock measures, much more helpful. This is true for the double reason that time existed and no doubt was perceived before the advent of clocks and secondly, because when we ask what it is that the clock measures we are back to our original question.

Such considerations in the absence of any more constructive material leads us to the tentative description of time as our perception of one of the two essential characteristics of movement. One of the essential characteristics is space and the other is time. This brings out the close relationship between time and space. Such an explanation smacks strongly of the theory of relativity and fits nicely with Gestalt theory in psychology. However, this need not necessarily constitute a criticism of the definition. Neither does it inevitably commit us to a purely Gestalt explanation of time since such time-space relations may be assumed to be the result of our experiences rather than nativistically given.

If we wish a more specific definition of time it may be described as the perception or probably better stated as the judgment of the amount of separation of two units or limits as greater or smaller, in terms of

some unit such as seconds. This judgment is reached from an estimation of how long it would take to accomplish so much or so many things such as to walk a block, listen to so much or to feel rested or fatigued by so much work.

While many theories have been proposed and many experiments conducted on the subject of time perception or time estimation* as yet there is no generally accepted view as to how we perceive or estimate time. Studies in time estimation might well be classified under several related but somewhat different problems such as,—

1. The estimation of relatively short time intervals. Most of the experimental work in time has dealt with the estimation of time between two signals up to a few minutes in length. In most cases the estimator has been forewarned that an estimation was to be made.

2. The estimation of time when the subject is not expecting to make such estimation. For example, if the reader was suddenly asked to estimate how long had been spent in reading this paper up to this point, we do not know whether the same methods of estimation would be used as in case he had expected to make this estimation.

3. It is also not certain whether the same methods are used in estimating and in reproducing time intervals. In estimating time a unit of estimate such as a second is necessary. In reproducing time, a unit is at least not so necessary.

4. The perception of longer intervals involving hours or days may be similar or may be different from estimating shorter periods of time. Certainly some of the same factors are involved but some new ones may also be involved.

5. There also is the question of how lower animals perceive time. Do they use the same methods as humans? At least there certainly is little "mental content" used in their estimations.

6. Then there is the problem of estimating the time of day or night. This at least in modern society involves direct reference to some instrument of time measurement such as the clock. In primitive society the sun or moon was, no doubt, generally used as the reference point. That our modern methods are only a refinement of this method is indicated by our usual methods of indicating time by so many hours, minutes and seconds since noon or midnight.

In the following sections of this paper we shall review the principal recent studies that have dealt with the problems listed above. Since, research has not proceeded as logically as the outline, this review will follow the various lines of research.

Since there have been excellent reviews of the literature on time estimation in a series of articles by Dunlap (13) from 1911 to 1916 and a later review by Weber (56) in 1933, this article will in general be limited to the literature since the later date. There will, however, be occasional

* The phrases perception of time and estimation of time are here used somewhat interchangeably. In a sense they are not strictly synonymous. Time estimation always implies a kind of quantification not necessarily included in the time perception.

violations of this principle (1) in order to emphasize some point of view developed during the earlier period and (2) in order to summarize studies in time perception in animals. This later topic was not included in the earlier reviews and it may well have an important bearing on research in time perception.

CUES FOR TIME PERCEPTION

One of the important problems in time perception is that of the cues used. In fact, most of the experimental studies in time perception can be related more or less directly to this problem. In the closely allied field of space perception the principal cues as well as their relative importance, as Carr (8) has so well pointed out, have been very carefully investigated. Although many studies have been conducted, no such list of cues has been discovered for time perception. In fact, almost the opposite is true. That is, we are hardly sure of any of the cues that are used in time perception.

Several of the earlier studies reviewed by Weber (56) investigated the relationship between certain physiological processes such as breathing, fatigue or digestive processes and time estimation. Some of these studies found evidence for such relationships but the evidence was never conclusive.

In some recent studies in this field Rosenzweig and Koht (45) compared time estimation and what they called "need tension." They defined need tension as a state of strain. They found that during periods of greater need tension there was more underestimation than when the tension was not so great.

A somewhat similar point of view was presented earlier by Francois (17, 18) and later by Hoaglund (30) who believed that time is mediated by internal temperature fluctuations. For example, Francois found that when the internal temperature was increased by high frequency currents, the tapping rate of the subject was increased. From such kinds of studies these two writers believe we have some kind of internal time clock for the perception of time.

Kawasima (37) had subjects estimate time while moving their arms in ten degree circles. Overestimation occurred when the action was easy and underestimation when it was difficult.

Shaefer and Gilliland (48) systematically varied (1) pulse rate (2) heart work (3) blood pressure (4) breathing rate and (5) lung work, in order to determine the effects of each on the estimation of time. No relationships were found between any of these variables and time estimation. Even when all these changes occurred together producing a

state of great physiological activity they seemed to have no influence on the direction of error or accuracy of time estimation.

Berman (5) investigated the relation of time estimation to what he called satiation. A stylus maze was used to produce satiation. When the subjects were satiated, 87 per cent of his subjects underestimated the time required for them to become satiated while 52 per cent of the non-satiated group overestimated the time required to reach their criterion of learning.

Jasper and Shogass (34) found that the alpha rhythm of the brain was in no way related to conscious estimates of time.

Certain drugs and disease affect the estimation of time. It is common knowledge that the drunk man is generally disoriented as to time as well as space.

Favilli (15) states that under mescal intoxication time estimation is very poor. Forty-five minutes was estimated as 15. But sometimes the estimates are abnormally short and sometimes abnormally long. Favilli explains this by suggesting that in the intoxicated state the shortening of the "field of consciousness" results in a sort of contemplation of the present instant which lacks a frame of reference. This seems to be a way of saying that the subject is always living for the present and lacks the usual perspectives for estimating time. If this be true, mescal intoxication may not have a greatly different effect upon time estimation than alcoholic intoxication. Marijuana also seems to produce a somewhat similar effect. It is sometimes smoked in cigarettes by orchestra leaders and drummers with the idea that it assists them in rapid timing. Whether it does or not seems not to have been investigated experimentally.

Sterzinger (49, 50) gave subjects several different drugs for the purpose of determining their effects on time estimation. After taking quinine subjects habitually underestimated 5 minute intervals. Alcohol produced too low an estimate for periods less than 25 minutes and too high for longer periods. Caffeine and thyroxin gave inconsistent results. Sterzinger came to the conclusion that time estimation depended upon metabolic rate.

FILLED AND UNFILLED TIME

One of the early topics to receive experimental study in the field of time estimation was the effect of filled and unfilled time. That is, what effect does the type or amount of mental content or physical activity have upon its apparent length? Although the literature of this topic is extensive the results, as will be seen from this survey, are by no means

consistent. Some results seem to indicate as William James (35) suggested that filled time while passing is perceived as longer than it really is. On the other hand many studies have obtained the opposite results. One explanation for such discrepancies is the fact that it is difficult to determine whether a time interval is filled or empty. The interval may be filled with a large amount of visual, auditory, or other type of external change. However, these may have little or no attention value for the subject who is estimating the time. On the other hand, the time interval may be relatively quiet so far as external stimuli are concerned and still be richly filled with mental content for the subject. For example, he may be thinking, "now one second, now two seconds, three seconds, etc." Many of the seeming inconsistencies in the results for different investigators are due to factors such as those just enumerated.

Much of the early experimental work on filled versus unfilled time was summarized by Triplett in 1931 (52).

Later studies by Helm (29) considered subjective factors such as attention as important in time estimation. Harton (26, 27, 28) found that activities of varying difficulty influenced time estimation. Time spent in making difficult discriminations was estimated as less than equal periods spent in making easy discriminations. In working at tasks in which the subjects were encouraged and felt they were being successful, the time was estimated as shorter than when they were discouraged and felt they were being unsuccessful.

There could hardly be more inconsistency in results than those found in the various studies in estimating filled and unfilled time. Does filled time appear longer or shorter than unfilled time? Does William James' dictum that filled time seems shorter in passing and longer in retrospect hold true? As already suggested, no single answer can be given to this problem except that time estimation depends upon other factors than whether the time is filled or unfilled. In so far as filled time may more often appear longer than unfilled time, it is largely a matter of the subjective filling of the time.

Recent studies by Abbe (1, 2) investigated the effect of increasing the amount of physical space upon time perception. Two blinking lights were used to delimit the time interval. The distance between these lights were varied but the time between their flashes was left constant. Abbe reports that time intervals were reported as longer when the space between the lights was greater. In a related experiment he also found the converse to be true, that is, if space intervals were given in a longer interval of time the space between the lights was judged as longer than when given in a shorter time. Tashiro (51) in a similar experiment in

which he could arbitrarily vary both the rate of tapping and the space between which the taps were made, concludes that the rate of tapping built up a gestalt with the result that the meaning of each tap was lost irrespective of the tapping speed. However, the tempo of the tapping which was determined solely by the spatial distance between the points struck in the taps was more powerful in producing the effect of gestalt movement than the rate of tapping. Rudert-Kotte (46) holds that binocular parallax, so important in space perception, may give clues to time perception through the space concept as well as through the slight time variants due to the fact that we see through two eyes instead of one.

While such studies as those just enumerated do not prove conclusively any theory of time perception, they give us material which must be taken into consideration in the formulation of any theory and they strongly indicate that there is a close relationship between the amount of mental content and the estimation of time. Space is directly related to time in that when space is extended this gives the impression of greater mental content. Yet this no doubt is entirely too simple an explanation of time and will need further elaboration as we proceed.

INDIVIDUAL DIFFERENCES IN TIME PERCEPTION

Three problems relating to time perception will be discussed in this section. One of these is the determination of the indifference point in time estimation. Another deals with the Weber-Fechner law and the other with sex differences in time estimation.

Most of the work on the indifference point in time estimation occurred before 1933 and was reviewed in Weber's (56) article. The results may be summarized by saying that different investigators have obtained very different results.

Similar inconsistencies have been obtained in determining whether Weber's law holds in time estimation. Some evidence indicated that errors are proportional to the length of the interval and time studies show no such relationship. In more recent studies Gilliland (20) using over 300 subjects with a total of about 5000 estimations ranging in length from 4 to 27 seconds found evidence favoring the Weber-Fechner law. But in another study by Gilliland and Humphreys (21) the per cent of errors was found to be significantly greater for the shorter than for the longer intervals. In this study the intervals ranged from 9 to 180 seconds. It seems likely that the difference in results in these later studies was due to the wider range of time intervals in the later study. It seems probable that since time is not mediated by any one sense but through several different senses no single constant could be established

in the Weber-Fechner formula. This may be especially true when the intervals vary greatly in length.

In the earlier studies of sex differences women were generally found to be poorer than men in time estimation. But in later studies such as those by Harton (26, 27, 28) and Gilliland and Humphreys (21) no such differences were found. It seems likely that the modern woman is called upon to make time estimates as often as men. Since time perceptions are primarily dependent upon learning the sexes probably do not differ in ability to estimate time.

TIME PERCEPTION IN LOWER ANIMALS

Some knowledge of how time is perceived may be gained from a study of time perception in lower animals. Because we do not believe that the lower animals, at least, use mental imagery to anything like the extent that humans do, any theory that is based upon mental content alone tends to be discredited.

The principal experimental studies of time perception in lower animals may be classified under the general headings of (1) delayed choice response, (2) delayed conditioned responses, and (3) time perception.

There is considerable doubt whether delayed choice responses should be considered under the heading of time perception. The delay of the choice response may not be due to an appreciation of time interval by the animal making the delayed response. Nevertheless, a brief account of such experiments will be included here since the length of the delay is one of the important factors in such reactions.

Hunter (33) using a multiple choice apparatus, measured the time that could elapse between the presentation of a light before the correct choice box and the release of the animal which would result in the animal going to the correct box. In a three choice box, Hunter found that for different rats the time could vary from one to ten seconds but the average time was not much more than one second. For dogs the time averaged about ten seconds and for raccoons it averaged about 20 seconds.

Walton (54) using a different kind of choice box obtained delays up to one minute in dogs. Warden and Warner (55) made tests on the noted dog Fellow and obtained delays of about 20 seconds between spoken commands and their execution. Yarbrough (59) obtained from two to four seconds delay in cats on a three compartmental and 16 to 18 seconds on a two compartment maze. But Corvan (9) obtained delays of 30 seconds with a Persian cat. In contrast with the previous studies Honzik (31) secured delays up to 45 seconds with rats.

The great variance of these results no doubt partly illustrates individual differences in animals but it also illustrates differences in methods used and apparatus. Because of these differences in results no very definite generalizations can be made other than to point out that the higher the animal in the scale, the more likely that the delay can be longer. It is also true that in the lower species the delay is maintained in terms of bodily posture whereas higher animals can change posture and still react correctly after a delay.

Several authors have studied time responses in invertebrates. Verlaine (53) taught wasps to discriminate between alleys of different lengths. He thought this showed a "concept of time." Braunschmid (7) made studies on time responses of fishes while Grabensberger (23) worked with ants and Sterzinger (49, 50) with ants, bees, and wasps. Richter (44) and Johnson (36) have studied the diurnal rhythms of rodents. Johnson found that this rhythm developed in wild mice born and reared in utter darkness. Beling (4) working with bees found that he could condition them to return for feeding any time of day or night.

Most of this work shows that some kind of relationship exists between diurnal rhythm, metabolic rate, and some sort of a time sense. But this sense of time is no doubt very different from time perceptions in humans.

Kuroda (40) by means of delayed conditioning found that rats were quite accurate up to about 20 minutes and that cats could be given delayed conditioning up to about 30 minutes. Anderson (3) experimented on time discrimination in the white rat. He used a multiple choice maze and confined the rat for different periods of time. Rats were able to discriminate between periods of 10 and 20 seconds. Kuo (39) constructed a maze with four pathways to the food box. Some were longer than others and in one the rat was confined for 20 seconds. The rats learned to go by the shortest route and avoided the one in which they were confined first of all. These results were further substantiated by an experiment by Sams and Tolman (47). Of course, it must be realized that other factors than time perception affect the order of elimination of pathways in such experiments.

Woodrow (58) taught two monkeys to differentiate between temporal intervals of 1.5 and 4.5 seconds. They were taught to reach for food upon the raising of a screen after the longer but not after the shorter interval. The monkeys became 90 per cent correct for these time intervals and 75 per cent correct in distinguishing between 1.5 and 2.18 seconds. Metfessel and Bobbitt (41) found that canaries could be taught to discriminate and produce rhythms according to a prearranged pat-

tern. They acquired an accuracy with an average deviation of only .01 seconds.

LEARNING IN TIME PERCEPTION

While some writers claim that the perception of time like other kinds of perception is largely dependent upon certain innate factors, probably no one would deny that learning plays a part in the process. The important questions are how does learning take place and how much of a part does it play. As indicated in the early part of this paper there are probably several different problems relating to time estimation. If so, then there are several problems related to learning in time perception.

The effect of different cultures on the development of time concepts is stressed in two papers. Kloos (38) states that the Persians stress the future while the Chinese stress duration, and the Indians stress the stagnation of time. Rodulescu-Motru (43) believes that the cultural effect amounts to a process of evolution whereby time is evolved through periodic changes in nature, in the body, and in culture. Thus he claims that part of the process of time perception is genetically inherited.

In the field of the experimental study of the time perception in children Elkin (14) reports that in children from ten to sixteen years of age there were large errors in time estimation. He found average errors of 82 per cent for short intervals up to 4.1 seconds and 57 per cent for longer intervals for the younger children. In sixteen year old children the error was 32 per cent for short intervals and 30 per cent for longer intervals. Gilliland and Humphreys (21) found average errors ranging from 32 to 47.5 per cent in fifth grade children as contrasted with errors of 15 to 31 per cent for adults. No doubt the greater accuracy in children in the latter study as contrasted with Elkin's study is due to the fact that Elkin studied Russian children from the home of workers whereas Gilliland and Humphreys studied American children from better homes. Such children have more occasion to be influenced by time and thereby learn the meaning of time intervals at an earlier age than the Russian children.

Several studies have investigated what sense departments are best suited for perceiving time. Goodfellow (22) found that, considering audition as 100 per cent, vision gave 65 per cent and the tactile sense 34. Other sense departments were even lower than any of these. Gault and Goodfellow (19) made two studies; one concerned with discrimination efficiency and the other with reproductive efficiency. In both the auditory sense was most efficient, vision second and touch was third. Forbenius (16) had subjects wake from sleep at previously arranged

times. He reports that they became able to do so within an accuracy of about five minutes. Wirth (57) taught subjects to reproduce given time intervals. Dudycha and Dudycha (11) asked 185 subjects to estimate the amount of time it would take them to perform six different tasks.

Gilliland (20) taught a group of 13 subjects to estimate time by counting. After each estimate they were told the length of the interval. In 25 to 50 practices these subjects were able to reduce their average error from 20 to 25 per cent to 5 to 10 per cent. A similar group with an equal amount of practice but without counting did not improve in their estimating.

THEORIES OF TIME PERCEPTION

Despite the large amount of disagreement in results in studies on time, many theories have been proposed to explain the perception of time. Most of the older theories postulated some kind of a time sense. The theory presented by Francois (17, 18) and Hoaglund (30) assuming the existence of some kind of an internal chemical time clock comes under this general heading. But later studies have not confirmed this point of view.

Psychoanalysts generally attribute time perception to some phase of the self. Hugenholz (32) explains time as follows: "The covert human time-form can exist only when there is a consciousness of self." Bonaparte (6) tells us that the child in whom the unconscious dominates has only a vague concept of time as in day dreaming and other forms of imagining. Dooley (10) explains time as an "Ego-defense properly placed in that class of defenses comprised of attachments to real objects in counterdistinction to narcissistic attachments." Just what is meant by such explanations and how convincing they are, is left to the reader.

Obendorf (42) explains time as "primarily dependent upon the reality with which recurrent physiological reflexes are registered. It becomes distorted in the absence of the sense of reality and in cases of profound interference with the perception of reality time sense may cease to exist. Another factor which is strongly influential upon time perception is the presence or absence of purpose, for without purpose the value of time greatly diminishes." Sterzinger (49, 50) as has already been stated believes time perceptions are based upon body metabolism, when metabolism is high, time passes quickly, when it is low, time drags. This bears a close relationship to the theory of Francois and Hoaglund and is essentially the theory proposed by du Nouy (12). Dunlap (13) stresses the relationship between time perception and bodily rhythm.

A number of writers emphasize the Gestalt point of view in time per-

ception. Weber (56) stresses the relationship between time and space. In a series of experiments by a number of *Gestalt* writers the close relationship between time and space is emphasized. This relationship is expressed by the formula "phenomenal velocity equals phenomenal space divided by phenomenal time or $v = s/t$." This is further emphasized in the statement "these variations in the flow of phenomenal time are not occasional examples which may be explained as illusions, but that they are continuous and are conditioned by almost any change in the structure of the entire field of movement."

While the evidence is either too conflicting or inconclusive to prove or disprove any theory of time perception, an analysis of the data lends support to certain general principles about how time is perceived. May we here sketch briefly some of these.

As has already been stated the evidence does not support a theory of any direct sensory basis for time perception. Unlike space perception and the estimation of distance time is less directly dependent on sense experiences as such. In the case of lower animals a discrimination of longer or shorter intervals and even some vague notion of time are, no doubt, mediated by physiological processes and body rhythm.

In children this vague awareness of time is further amplified by learning units of time such as a second or hour. This knowledge is gained either directly or indirectly from timepieces. The significance of these intervals are interpreted in terms of body tensions and rhythm. As the child gains further experience it learns more about evaluating how much activity can occur in any given time. The sensory cues can come from any sense modality and the resulting activity may be physiological or mental.

In the case of the rat or dog in the delayed conditioned response it is certainly mostly physical. In the case of the reader in estimating how long it has taken him to read this paper his estimate is probably based upon his estimate or knowledge of how many pages he has read and at what rate he reads a page. Likewise if a person knows that he ordinarily walks about five miles per hour when he has walked what he thinks is about three miles he will estimate that he has been walking for 40 minutes. If a subject is not paying attention to what is going on during the interval or if he has no good idea of the amount that has taken place or of units for measuring the interval, his estimates will be poor. Training as shown in learning the normal rate of one's counting during an interval will soon make a person able to estimate time very accurately. Without some kind of definable content during the interval, estimation remains poor.

In retrospect estimation depends upon the memory of events occurring within any interval. If the interval is filled with many events it will seem long in recall. If it is uneventful it will be remembered as short. Similarly in immediate estimation the estimation will depend upon the amount that has occurred or seemed to have occurred during the interval. It is for this reason that when we are doing interesting things time is generally underestimated. In waiting or boredom time passes slowly. However, there is always a correction that the estimator may make. If a person knows that time is underestimated during pleasant happenings and overestimated in boredom he may correct or often overcorrect for these influences. It is, no doubt, in large measure because of these facts that the literature on filled and unfilled time is so hard to interpret.

According to this theory time estimation is not nearly so directly given from sense data as in space perception and estimation. Time estimation partakes more of the nature of a judgment rather than a perceptual process. The fact that in space perception the cues remain for further examination whereas in time perception after the event it is always in retrospect, helps to make time estimation more of a process of judgment than of perception.

What has thus far been presented as a brief theory of time perception applies particularly to short intervals. Possibly longer intervals are estimated in somewhat the same way but more often with greater reference to external events such as the position of the sun and familiar events such as the amount of traffic on the street as in estimating the time of day. Of course, when a timepiece is available it will be used.

In an earlier part of this paper six general fields of research in time perception were indicated. These are areas in which further research could add much to our knowledge about time. In addition to these general fields of research much insight might be gained about the nature of time from a further study of time estimation in animals. The evolution and improvement of time perception both in children and adults would also, no doubt, throw further light on this subject. Such studies would have both practical value in showing how to train persons in time estimation and would extend our knowledge about the psychological nature of time.

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BOOK REVIEWS

STEINER, LEE R. *Where do people take their troubles?* Boston: Houghton Mifflin, 1945. Pp. xiii+265.

What do we psychologists do when our personal problems become so severe we are ready to jump off a tall building or take other drastic action? More adequate adjustive behavior involves consulting with a respected colleague or a psychiatrist. Mrs. Steiner is concerned with what the great gullible public does with its problems. Her book indicates that a surprisingly large number of otherwise fairly intelligent people consult such purveyors of psychotherapy as newspaper columnists, radio "consultants," marriage or "get acquainted" bureaus, religious faddists, spiritualists, palmists, and numerologists. These "therapists" compensate for their lack of formal training by assuming an air of great self-confidence and something approaching omniscience.

The book begins with a useful discussion of "What is a psychologist?" It is pointed out that there are recognized courses of training preparing people to be psychiatrists, psychiatric social workers, psychoanalysts, and clinical psychologists. Examination of telephone directories and contacts with license bureaus and health departments in big cities showed an almost complete lack of regulation of people dispensing psychotherapy as long as they avoided hypnosis and the treatment of serious mental disorders.

What sorts of therapy and therapist appear in this wide open field? It is easy to discount "graduates" from the "College of Divine Metaphysics." This institution confers a "Doctor Degree in Psychology" on the basis of two courses, costing fifty dollars each. A surprising thing is the large number of similar institutions giving "degrees." A common characteristic of most of the "therapists" discussed by Mrs. Steiner is their lack even of one of these dubious degrees—one simply adds to one's name a set of letters like Ms.D. or Ps.D. After all, why bother to go to school when your practice is based on common sense, of which you clearly have more than your share?

As a circulation-builder the newspaper columnist-consultant is probably useful to the publisher. However, the advice given may often be harmful and is rarely helpful. Mrs. Steiner sent a copy of the same letter to a number of leading columnists, briefly outlining a mother's problem with a disobedient child. One advocated sending the boy to a special school, and several others proposed stern discipline of one sort or another. Similarly, advice based on inadequate evidence is freely given on a number of radio programs listened to by millions. Both types of "advisors" have in common a certain incompetence and an engrossing interest in the commercial aspects of their ventures.

The alleged therapist who deals with an office clientele is most ob-

viously the profiteer on others' emotional problems. Mrs. Steiner devotes the greater part of her book to a carefully documented report of her experiences with these people and their "patients." We can visit with her the "Human Audit Bureau" where we are given vocational guidance on the basis of our brain development as measured with special calipers. If we need to find companionship of a sort, we can always contact a marriage broker or some introduction service. After all, "Why let lonesomeness rob you of romance, happiness, and comfort?" What shall we do if we are really interested in religion but are not satisfied with the regular recognized churches because they don't seem to solve our personal difficulties? This problem is "easy." There are many local "churches" to take care of us—would we prefer spiritualism mixed with a little hypnosis, or stick with the "I Am" system? Perhaps we are prejudiced in favor of something like palmistry, graphology, astrology, numerology, or tarot; many practitioners of these occult sciences parade through the chapters self-confidently if somewhat denuded of respectability.

Mrs. Steiner is not one of the smoothest of writers. This reader's sensation was often one of bumping along from one section to another with little help in transition. The overwhelming mass of apparently scrupulously recorded and reported experiences making up the book's documentation probably contributes to this feeling of disjointedness. However, the subject material of the book will be of great intrinsic interest to any professionally-minded psychologist, and a careful reading will repay anyone with even the glimmerings of a "social conscience."

A review would be incomplete without at least a mention of some of the book's obvious possibilities for professional use. One could:

1. Recommend it to his more intelligent students in elementary or clinical psychology courses as supplementary reading material.
2. Pass it along as encouragement or reminder to committees in professional organizations engaged in formulation and acceptance of standards to be met by practicing applied psychologists.
3. Use it to strike lightly those psychologists who have obstructed the setting up of such professional standards.
4. Bring it to the attention of legislators and point out similar conditions existing in their own constituencies, hoping that legislative support may thus be obtained for regulation of people unwarrantedly calling themselves psychologists.
5. Write letters quoting its conclusions to newspapers and periodicals accepting advertisements from individuals and organizations similar to those discussed.
6. Pass along the information from it in any ethical, practical way to the public which has so often been victimized.
7. Use of an occasional rereading of sections of the book to reawaken one's

conscience and remind oneself that such abuses do exist, and that it is the professional psychologist's duty to eliminate them.

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KAPLAN, OSCAR J. (Ed.), *Mental disorders in later life*. Stanford, Calif.: Stanford Univ. Press, 1945. Pp. vii+436.

The disparity between the available research on the early segment of development (childhood), and the late (senescence), has long been obvious. The paucity of factual material available on mental decline has no doubt been disquieting to the teacher of developmental psychology who strove to present consistently the entire sequence. Particularly is this void conspicuous when aging is viewed as the ultimate consequence of maturation and integration. Moreover, this period of the individual's growth assumes increased importance since more persons now live to be old, thus steadily increasing the ratio of old people to those in other age brackets.

The editor modestly presents the seventeen chapters as a collection of essays and not "a full summary of the vast literature already available in this field." Although the title and general orientation of the volume emphasizes mental disorders, several of the chapters are good, up-to-date reviews of typical changes with aging.

In these days of specialism it is heartening to find men of diverse scientific backgrounds collaborating on the treatment of phenomena of a limited field, even though this cooperative effort is additive rather than integrative. A brief, general introduction of the volume by a psychiatrist (Karl M. Bowman) is followed by a review of later life disorders by a statistician (Horatio M. Pollock). Well documented systematic articles on the physiological (Nathan W. Shock) and psychological (Harold E. Jones and Oscar J. Kaplan) aspects of normal and abnormal age changes follow. To complete the survey of older age processes the newer and highly significant sociological (H. Warren Dunham) and nutritional (L. Erwin Wexberg) approaches are added.

Chapters on the *Neuroses* (Norman Cameron), the *Involucional Psychoses* (Eugene Davidoff), the *Presenile Dementias* (George A. Jervis), the *Senile Psychoses and Psychoses with Cerebral Arteriosclerosis* (David Rothschild), the *Aged Subnormal* (Oscar J. Kaplan), and the *Toxic Delirious Reactions* (G. Wilse Robinson, Jr.), outline the clinical disorders of this age period with an emphasis on the recent literature. *Psychotherapy* (Fred V. Rockwell) and *Mental Hygiene* (Nolan D. C. Lewis) each merit a chapter in addition to sections on treatment in the clinical chapters. A systematic chapter ends the collection. It is written by a gerontologist (Edward J. Stieglitz) who has a broad perspective

on the phenomena of aging and its unsolved problems. The chapter does not, however, specifically articulate with those preceding it. Sandwiched between the clinical types is a novel chapter describing a study in one institution of *Older mental patients after long hospitalization* (Eugenia Hanfmann).

Like most volumes of this kind, the style, general outlines, extent of organization of facts and completeness of coverage of the materials vary greatly. The editor does not state the degree to which each writer was cognizant of the other contributors' outlines or manuscripts. However, the overlapping and repetitions that occur do not detract from the book.

The contributors differ in the extent to which they deal in mere description and in hypothetical explanation. Some approach their phenomena from an atomic and some from a multiphasic standpoint. Wexberg, in treating nutrition, finds it necessary to discuss employability of the aged. In a discussion of neuroses, Cameron generalizes more as to the basic nature of psychological changes than do the authors of the chapter devoted to that subject. Shock integrates his detailed, scholarly chapter, based upon 281 titles, and attempts to explain impairment of mental functions by the hypothesis that aging is attributed to progressive loss in the homeostatic capacities of the organism. Dunham, with less data, presents a framework to interpret the interplay of culture upon the biological factors.

This factual volume is a distinct contribution to abnormal and developmental psychology. Dr. Kaplan is to be commended for his broad perspective of the problem, his appeal to specialists for data, and his excellent choice of contributors. However, with full cognizance of the enormity of the task and the potential vulnerability of the author, one does regret that a chapter has not been added to attempt to integrate all of the contributions.

FRED MCKINNEY.

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SHERMAN, M. *Intelligence and its deviations*. New York: Ronald Press, 1945. Pp. x+286.

The first part of this brief text describes the nature of intelligence, mental growth, and the relation of mental growth to physical and environmental factors. There follow brief discussions of intelligence in delinquency and psychosis, but most of the discussion of deviations concerns mental deficiency in its association with neurological disorders. There are brief sketches of intelligence tests, of genetic problems in intelligence, and of intellectual superiority.

The treatment of the topics is very uneven. The author seems at home in the medical discussion of such conditions as cretinism, microcephaly, birth injury, epilepsy and hydrocephalus, and the information,

presented rather technically, will be of interest to the psychologist. There is a chapter on the adjustment of the mental defective which, while short, surveys a number of studies. As a general text, however, this book is not satisfactory.

In a general discussion of intelligence and its deviations, which it is the author's stated intention to provide, one would expect a well-rounded development of the many facets that the subject possesses. Yet, in this very brief book, there is a very meagre treatment of most of the topics, and some important subjects are not mentioned at all. One feels that Professor Sherman wished to present the medical and neurological aspects of certain conditions which occur in some defectives, and then added material deemed necessary to prevent the book's having a rather limited and specialized appeal.

There is a very brief and inadequate discussion of the rationale of intelligence testing, a knowledge of which is indispensable to the systematic understanding of the field. There is very little mention of qualitative features of test performance, test patterns, problems of rapport, differences between diagnostic groups (some of which are never mentioned), or of many other significant and important topics. From the book, one would conclude that the only really significant deviation from normal intelligence is in the direction of deficiency. Seven pages are devoted to the intellectually gifted! In the chapter on intelligence and delinquency, the major problem raised is whether the average intelligence of delinquent groups is below that of other groups. Only eight pages are devoted to intellectual changes in psychoses other than the psychoses of mental defectives, and little of the extensive literature in this area is cited. Few of the basic problems studied are made known. It is a sterile and rather futile picture of the study of intellectual deviation that is revealed in these pages. None of the studies of intellectual function of such investigators as Goldstein, Hanfmann and Kasanin, N. Cameron, J. Hunt (to mention but a few) is mentioned, and the whole problem of test performance in relation to lobectomy is ignored.

The emphasis in presentation is well illustrated in the chapter on epilepsy, to which 19 pages are devoted. Three pages are given to intellectual changes (deterioration). The nature of epilepsy, its age of onset, personality changes, and its pathology are described, and one derives a reasonably adequate picture of the disease. But this is a book about *intelligence* and *its* deviations.

The discussions of general topics, such as theories, growth, and the like, are poorly organized and will hardly leave the student with a clear idea as to what the problems are or what has been done in their solution. Professor Sherman feels, for example, that IQ differences found in identical twins reared separately are an indication of "achievement levels" dependent upon different environments. This, of course, is the crux of the matter. But he then argues that such results cannot evi-

dence the influence of environment on intelligence, because tests are not culture free (253). Is the concept of intelligence itself culture free?

There are a glossary of terms, mostly medical, and a bibliography of over 300 titles. It is perhaps significant that but seven titles bear publication dates later than 1939.

CHARLES N. COFER.

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BRENNAN, ROBERT E. *History of psychology, from the standpoint of a Thomist*. New York: Macmillan, 1945. Pp. xvi+277.

The author presents and evaluates the history of psychology from the point of view of Thomas Aquinas, whose views in turn were based upon the writings of Aristotle. Nearly half of this condensed history is devoted to the antecedents, philosophic and otherwise, of psychology as a separate field. The remainder of the book is devoted to an accurate but very brief presentation of the important events, research, and personalities in the development of modern psychology. The following is evidence of the brevity of the presentation: Wundt, and his achievements, are presented in 4 pages, although there are references to his name or work on 18 other pages; William James is given 3 pages and 4 other references; while Freud is given 13 pages and 9 other references.

Historical sequence is carefully followed *within* the development of a topic, but the omission of dates limits the possibility of temporal cross reference from one topic to another. This is compensated for in part by the inclusion of a bibliographical index which gives the years of birth and death as well as listing the chief publication of the individual by title.

The author's viewpoint is epitomized in the following paragraph,

What we need today, as Aquinas would contend, is really less of psychology and more of anthropology, using the term "anthropology" in its traditional meaning to signify the study of man, as man, not as a concatenation of reflexes, or a sum of perceptual configurations, or a series of imaginal processes or a complexus of instinctive responses. Such things are simply isolated events in the history of human nature, and they have no meaning except in relation to the whole nature. Further, the study of man, as man, means the study of man as a besouled organism, or as a creature composed of matter and spirit, whose operations fall within the dimensions of scientific analysis, but whose fundamental nature is the proper study of philosophy. (257)

The research man will probably find the material of this book too condensed and the references too limited to serve his interests. Many an academic man will object to the philosophical and religious emphases as being too extensive for his effective use in the classroom. However, considering the religious and philosophic background of the author, most psychological readers will be gratified to observe the emphasis

placed on and the respect shown toward the *established scientific observations* in all fields of psychological investigation.

WILSON McTEER.

Wayne University.

TRAXLER, A. E. *Techniques of guidance: tests, records, and counseling in a guidance program*. New York: Harper, 1945. Pp. xiv+394.

This comprehensive survey of methods and procedures for use in guiding and counseling high school students is written for the benefit of high school teachers and administrators. The book includes (xiii) "much detailed explanation concerning tests and other instruments of evaluation and a large number of illustrative record forms," written to advise school personnel who may have had little psychological training. Detailed and pertinent suggestions on a variety of associated administrative problems are also provided. Such a book is to be evaluated not as a scientific treatise but as a guide to the proper application of psychological techniques and procedures.

Apart from a brief discussion of occupational opportunities early in the book and an equally brief treatment of "adjustment" problems at the end, the primary concern is with problems of distribution or placement of students within the educational system. The book is organized around the individual cumulative record, which is to include information obtained periodically for each student in the following ten areas: home background, health, out-of-school experiences, plans for the future, school history and record of class work, mental ability or academic aptitude, special aptitudes, achievement and growth in different fields of study, educational and vocational interests, and personality. Information in the first four areas can be obtained from each individual by interview or questionnaire, and school records accumulate more or less automatically.

Information in the last five areas involves the use of tests and observational procedures, and it is here that a detailed explanation of methods of obtaining and applying such information is needed. A minimal explanation of the applicability of a measure in a guidance situation should include a statement of the validity of the measure in predicting scholastic, vocational, or other performance, so presented that a statistically unsophisticated person could grasp the actuarial nature of the problem. This we are nowhere given.

The chapter on the appraisal of aptitudes describes or mentions a number of standard tests of intelligence or general academic aptitude suitable for use with high school students. A carefully prepared annotated list of thirteen such tests, which is presented at the end of the chapter, gives for each test the author(s), publisher, cost per copy, mention of the component parts, and reliability coefficients. For only one of the thirteen tests is validity in relation to scholastic performance

mentioned at all. Since tests of this type do have substantial validity for many purposes, and considerably less validity for many others, it would seem that a more intensive consideration and explanation of this subject would lead to a more discriminating use of such tests.

Under "special aptitudes," similar information is given for tests of art, music, science, mechanical, clerical, mathematics, and foreign language aptitudes. Counselors are warned that the label of a specific aptitude test does not always indicate accurately what the test actually measures, but methods of choosing the most effective of several similarly named tests are not provided.

A chapter on the evaluation of achievement presents an exhaustive annotated list of achievement tests in all scholastic areas.

In presenting a catalogue of personality tests, questionnaires and interest inventories, the author is carefully critical of some of the procedures reviewed; others are simply mentioned or described briefly. (107) "Although there have been many studies of personality tests, the validity of the various tests in this field has not been established." Thus the precautionary statement earlier (100), "Before introducing a new test for measuring personality, a school will do well to try it out experimentally with a few pupils to see whether or not it contributes to the description of the personality of the individual pupil."

Several rating scales are described with a caution about "halo effect." Much more attention is given to the "anecdotal record," a systematic collection of anecdotes "concerning some aspect of pupil behavior which seems significant to the observer." No attempt is made at a statistical evaluation of this anecdotal procedure, either as to inter-observer reliability or as to the prognostic value of various types of behavior samples.

In a separate chapter, a large number of uses to which objective test results may be put are described. A discussion and proposed solution of the validity situation is given as follows (199):

Research is the only means by which the prognostic nature of a test can be adequately investigated, but research techniques are often slow and expensive and not enough time has elapsed since the publication of the majority of the good tests for research to discover their relationships to fundamental long-time objectives. . . . Personnel workers in schools, however, cannot wait until research with tests has supplied answers to many of their questions, for they must meet today the problems of guiding their present students. Nor is it necessary for them to wait, for they can read a great deal of meaning into the scores on the basis of their own experience.

The reviewer believes that if some of the effort being spent on reading meaning into test results could be channeled into research on their relationships to objectives, the number of areas in which we read meaning *from* rather than *into* scores could be increased.

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YALE, J. R. *Frontier thinking in guidance*. Chicago: Science Research Associates, 1945. Pp. 160.

This book is an anthology of 24 articles in the field of guidance. All except one of the papers has appeared elsewhere and are reproduced in this volume because, in the opinion of the editor, they "are selections written fairly recently which the well-informed person should know or know about" and are "recommended reading of definite interest to the guidance person." Each of the 24 contributors (13 of whom are psychologists) is a recognized authority and has made significant studies in this field.

There are six parts to the book: *Part one, Education prepares to increase guidance*, contains a discussion of needed reforms in the secondary school system (Reeves), and presents proposals of the National Resources Planning Board (Blaisdell) for increased federal support and of the New York Regents (Wilson) for increased state support of secondary education; *Part two, Guidance comes of age*, contains articles on the genesis of modern guidance (Paterson), the growth of occupational testing (Borow), the relation between guidance and instruction (Brewer), evaluating the effectiveness of counseling (Wrenn and Darley), the importance of guidance in the elementary school (Addy) and includes a summary of the statement prepared by the ACE Committee on Problems and Plans in Education on student personnel services; *Part three, School guidance programs in operation*, discusses plans developed in Glencoe, Illinois (Kawin), Philadelphia (Boyer) and San Francisco (Schmaelzle); *Part four, Undertaking readjustment of the veteran*, presents material pertinent to the counseling of veterans in the post war period (Williamson), accrediting veterans returning to school (Jacobson) and two states—Connecticut and Michigan—veterans' programs (Gray, Fern, Horn); *Part five, Tools for the guidance worker*, includes articles on non-directive counseling as an effective technique (Rogers), a survey of occupational filing systems (Yale), occupational analysis activities in the War Manpower Commission (Shartle and Dvorak), a critical analysis of textbooks surveying the field of guidance and student personnel (Fredenburgh) and a minimum library for counselors (Smith); and *Part six, The counselor obtains additional training*, contains sections devoted to laboratory training for counselors (Lloyd-Jones), selecting and training teachers—in Fargo, N. D.—for individual guidance (Froehlich) and short term training programs for counselors (Jaqua).

Obviously with such a miscellaneous collection of topics it is difficult to arrive at an adequate overall evaluation. The articles were prepared quite independently of any thought of inclusion in the volume under review, so duplicate each other in minor ways and do not of course constitute a systematic treatment. Certain of the articles are critical in content whereas others are merely descriptive. They are interesting but undoubtedly many similar volumes of completely different articles could have been prepared, so one may ask, why *this* particular collection?

There is no adequate discussion of this point, nor is there any introduction or summary that brings together the points which the editor may have thought significant. It would seem that if the separate articles are worth presentation in a single volume (and the reviewer is not doubting this) they should also be worth an adequate summarization in some chapter designed to integrate the ideas involved. If the title were less pretentious and something like "A few readings in the field of guidance" the contents would be much more accurately described. This is not to deny the editor's contention, however, that some of the articles included do represent significant and critical thinking in the sense which the title implies.

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DUNSMOOR, CLARENCE C. (Chairman) for New York State Counselors Association. *Practical Handbook for counselors*. Chicago: Science Research Associates, 1945. Pp. 160.

Although this handbook was prepared to assist high school counselors, it will be of interest and value to high school teachers and counselors, college and university counselors, teachers college faculty who teach courses in guidance or school administration, and to school administrators or boards who are responsible for authorizing guidance programs and selecting counselors.

An outstanding merit of the handbook is the comprehensiveness of the guidance program which it outlines. It outlines a program extending from the elementary school through follow-up studies of post-school adjustments and including educational and vocational guidance, psychological testing, personal adjustment counseling, study methods, and orientation to the school. Its major deficiency is to be found in the area of actual guidance methods. That is, the handbook presents an excellent outline of the administrative procedures, tools, and techniques of collecting data to be used in guidance but does not go very far in telling the counselor how to use the data in guiding the individual. For example, several psychological tests are listed but very little comment is made on how to interpret or use the test results; several study skills are listed but no suggestions are given as to methods by which they can be developed; and the techniques and content for a case study are outlined but no suggestions made as to how to use or interpret the data collected. It appears that the authors assumed that the counselor who uses the handbook has been trained in guidance methods and psychology but can profit by a comprehensive review of the scope of work that he should include in his guidance program. In spite of this deficiency,

the handbook deserves a distinctive position in the literature regarding guidance programs in the elementary and secondary schools. If a school system organized a guidance program of the scope outlined in this handbook and secured adequately trained personnel to do the guidance work, it would have an excellent program. However, one administrative arrangement the handbook omits is for the counselor to have access to psychological or psychiatric assistance in the diagnosis and treatment of behavior problems, nor does it recommend that the counselor be trained in psychological therapy, abnormal psychology, or mental hygiene.

The handbook contains eighteen chapters, which are very brief, averaging seven pages per chapter. The chapters cover such topics as: the role of the counselor, the objectives, scope, and content of the guidance program, technique of the counseling interview, the maintenance of cumulative records, tests of abilities, achievement, interests, and personality, content and techniques of the case study, group activities as a part of the guidance program, orientation and adjustment to the school, course selection and program planning, vocational guidance, placement program, preparation for college, financing a college education, follow-up studies of post-school adjustments, the counselor's personal qualifications, the counselor's relationships with other persons and agencies, the counselor's tools, and the counselor's professional development.

Chapter 17, the *Counselor's professional tools*, contains an excellent bibliography of books, journals, and other publications dealing with youth problems, guidance techniques, specific phases of guidance, and occupational and educational information. At the end of the handbook is a bibliography of additional books on various phases of guidance and a list of agencies which establish the qualifying requirements for various occupations and professions.

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TRIGGS, FRANCES O. *Personnel work in schools of nursing*. Philadelphia: W. A. Saunders Co., 1945. Pp. xv+237.

The "personnel point of view" is doubtless as applicable and as efficacious in the field of nursing training as in any other area of education. In fact, a very considerable amount of work has been done in this area, as is evidenced by the bibliography of 354 titles included in the present book. The book itself is in the nature of a general survey of the field of student personnel work with particular reference to the problems of student nurses.

The general treatment of the topic is rather eclectic with a generous infusion of the "newer approach to psychotherapy" sponsored by Rogers

and by the Elliotts. The consequences of this eclecticism are evident in a conflict between statistical objectivity and clinical case-work.

The book starts with a sketchy exposition of *Human Behavior and Adjustment* which runs the gamut of physiology (the autonomic nervous system and the endocrines) emotion and motivation, learning and conditioning and the psychology of adjustment—all in 23 pages. This is followed by five chapters on *The Counselling Program* (educational, personal and vocational), and three chapters on testing and personnel records. The final chapter, entitled *The Story of a Student*, sets forth a complete case history in some detail.

The treatment is rather uneven. The sections on counselling problems are written in a style that is clear and straightforward, if somewhat too cursory. In handling the more technical topics of testing and statistical analysis, however, the author often descends to a "popular" level which is confusing at best.

Altogether, the book presents a sympathetic and (for the most part) readable account of the work of the counsellor in a school of nursing which should serve as a stimulus to "start the prospective professional counsellor . . . on the road to her technical training." Its major weakness lies in its failure to provide a rational basis for the predictive process and its reliance on an uncritical amalgam of any and all information arrived at through the somewhat mystical medium of the counselling interview.

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